AMERICAN SOCIETY OF CIVIL ENGINEERS.
INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

360.
(Vol. XVI.—June, 1887.)

A MASONRY DAM.

By JOHN W. HILL, M. Am. Soc. C. E.

READ SEPTEMBER 15TH, 1886.

WITH DISCUSSION.

Eden Reservoir is the principal settling and distributing basin of the Cincinnati Water-works; contains two divisions, one of 57 000 000 gallons, and the other of 43 000 000 gallons nominal capacity; and receives a daily pumpage from the Ohio River of 15 000 000 to 20 000 000 gallons.

The site of the reservoir was a natural ravine, across which were built two masonry walls, one forming the division wall between the two sections or basins, and the other the main retaining wall, or dam, placed lower down in the course of the ravine.

The ravine was filled with earth, and the filling covered with a concrete floor 12 inches thick (in both sections of reservoir), to a level within 29 feet of the crest of the division wall, and within 35 feet of the coping of the main retaining wall.

In addition to filling the ravine, there was considerable excavation of the slopes of the upper basin to increase the capacity of the reservoir, and the slopes (2 horizontal to 1 vertical) were all paved with limestone blocks, set in cement-mortar.

The natural soil in the ravine was excavated to the rock and marl strata; leveled off and stepped up the sides of the natural slopes as

shown in Plates XX and XXI, and the walls built of lime-stone obtained from the neighboring hills.

The work was commenced in January, 1866, and the division wall and basin above it completed during 1872; but no use was made of it until October, 1874. The main retaining wall and the lower basin were finished and put in service late in 1878.

As shown by the plan of the reservoir (Plate XIX) the basins are of irregular form, and the slopes follow the natural contours of the ravine, excepting where the excavation in the upper basin somewhat changed these to increase the storage room.

Under the floors of the reservoir, sewers were laid in the natural soil to intercept and conduct away the subterranean drainage; these united in a single outfall sewer of stone masonry of 5 by 6 feet diameters, which conducts the drainage into the Ohio River.

The division wall was constructed with an influent chamber at one end and an effluent chamber at the other, between which the length of wall at the crest is 307 feet.

This wall (Plate XX) is trapezoidal in section; has a width at crest under coping stones of 10 feet; a height at center of 67.5 feet, and a base width at same point of 30 feet.

The division wall has a batter alike on both sides, and is of uniform stability and strength when subjected to the lateral pressure of the water in either section of the reservoir, with the opposite section empty. Under ordinary conditions of service the depth of water in the reservoir is 30 feet, or 1 foot over the crest of division wall.

That portion of the division wall above the floors of the reservoir is of uniform section throughout its length, and has a top width of 10 feet, a bottom width of 19 feet, and a height equal depth of water of 29 feet.

Estimating the weight of the masonry at 144 pounds per cubic foot, and the coefficient of friction as 0.60, the vertical pressure on the horizontal joints; then the frictional stability of the upper 29 feet of wall, with one section of the reservoir full and the other empty, is

$$\frac{10+19}{2} \times 29 \times 144 \times 0.60 = 36331.2 \text{ pounds}$$

per unit of length, and the horizontal thrust of the water is

$$\frac{29^2 \times 62.5}{2} = 26281.25 \text{ pounds},$$

and the ratio of frictional stability to horizontal thrust of the water,

neglecting the cohesion of the mortar in the joints, is

$$\frac{36331.2}{26281.25} = 1.38.$$

Neglecting the vertical component of the prism of water sustained by the batter of the wall, and taking the center of gravity in the vertical center of the wall, the leverage resistance is

$$\frac{10+19}{2} \times 29 \times 144 \times 9.5 = 575\,244 \text{ pounds},$$

and the overturning moment,

$$\frac{29^2 \times 62.5 \times 29}{2 \times 3} = 254\,052 \text{ pounds},$$

and the ratio of leverage resistance to overturning moment =

$$\frac{575\,244}{254\,052} = 2.225.$$

Of course the vertical pressure of the water on the battered surface of the wall, and the cohesion of the mortar in the joints, have an actual effect to increase the stability of position as well as to increase the frictional stability.

According to the method of the French engineers, which neglects the frictional stability and stability of position, but fixes the strength of the wall at any bed joint by the pressure developed at the extremity of that joint remote from the thrust, we have under same conditions of loading the following result.

Vertical component of the prism of water resting upon the batter of the wall, 4 678.125 pounds.

Horizontal position of the center of gravity of the prism of water from vertical line cutting the wall 29 feet from the crest = 1.5 feet.

Weight of the masonry in wall (above joint) 29 feet below crest, 60 552 pounds.

Horizontal ordinate of center of gravity of the section of wall,

$$x = \frac{1}{3}\left(a + b - \frac{ab}{a+b}\right)$$

where a = width of wall at top; b = width of wall at given joint, and x = horizontal ordinate of center of gravity. Inserting values and working,

$$x = \frac{1}{3}\left(10 + 19 - \frac{10 \times 19}{10+19}\right) = 7.483 \text{ feet}.$$

Vertical ordinate =

$$Y = \frac{H}{3}\left(\frac{2a+b}{a+b}\right)$$

where H = vertical depth, or depth of wall to given joint, and Y =

vertical ordinate from given joint to center of gravity. Inserting values and working,

$$Y = \frac{29}{3}\left(\frac{2 \times 10 + 19}{10 + 19}\right) = 13.000 \text{ feet.}$$

Horizontal distance at depth 13 feet from floor of reservoir, from face of wall to vertical line bounding the water prism,

$$\frac{4.5 \times 13}{29} = 2.017 \text{ feet,}$$

and, horizontal distance of center of gravity of section of wall from same vertical line =

$$7.483 + 2.017 = 9.5 \text{ feet.}$$

Horizontal distance of center of gravity of the combined prism of water and section of wall from vertical line bounding the water prism =

$$x = \frac{w x_a + W x_v}{w + W}$$

Where w = weight or vertical component of the prism of water,
" W = weight of the section of masonry above given joint,
" x_a = horizontal ordinate of center of gravity of water prism from its vertical boundary,
" x_v = horizontal ordinate from same vertical to center of gravity of section of masonry,
and x = horizontal ordinate of center of gravity of combined prism of water and section of masonry;
inserting values and working,

$4\,078 \times 1.5 =$	$6\,117$
$60\,552 \times 9.5 =$	$575\,244$
$64\,630$	$581\,361$

and,

$$x = \frac{581\,361}{64\,630} = 8.995 \text{ feet,}$$

and horizontal distance of center of pressure from center of gravity =

$$x_1 = \frac{P h}{3 W}$$

Where P = horizontal component of the water pressure at face of wall,
" h = depth of water,
" W = weight of masonry,
and x_1 = horizontal distance from center of gravity to center of pressure; inserting values and working,

$$x_1 = \frac{26281.25 \times 29}{60\,552 \times 3} = 4.195 \text{ feet.}$$

The center of pressure is distant from the edge of the join

$$u = 19 - (8.995 + 4.195) = 5.81 \text{ feet},$$

and the resultant pressure per square foot at edge of joint is obtained from one of the two expressions,

$$P_y = \frac{2}{3}\left(\frac{W}{u}\right) \qquad (A)$$

or,

$$P_y = 2\,(2t - 3u)\frac{W}{t^2} \qquad (B)$$

Where W = weight of masonry above joint,
" u = distance from center of pressure to extremity of joint,
" t = width or length of joint from face to back of wall,
and P_y = pressure per square foot at extremity of joint.

(A) applies when $u < \frac{t}{3}$

(B) applies when $u > \frac{t}{3}$

Substituting values we have, for

$$P_y = \frac{2}{3} \times \frac{60552}{5.81} = 6948. \text{ pounds},$$

or 48.25 pounds per square inch.

The limiting pressure, according to French authority, is 92.5 pounds per square inch, although this pressure is largely exceeded in many existing works.

This wall has frequently heretofore, and is at the present time, required to act as a dam with the upper section of the reservoir in service and the lower section empty.

The pressure at the base of the wall, including the thrust and counter-thrust of the earthfill, taking the angle (φ) which the plane of fracture of the earthfill makes with a vertical cutting the base of wall—at 25°, the weight of the material in the fills at 100 pounds per cubic foot, and the vertical depth of a mass of earth the base load of which, per unit of area, equals the vertical pressure of a column of water 29 feet high or deep, as $\frac{29 \times 62.5}{100} = 18.125$ feet, and depth of fill assumed for calculation $= (67.5 - 29) + 18.125 = 56.625$ feet.

Then the horizontal component of the earth-fill on the water side of the wall is,

$$P_3 = \frac{h_1^2 \times tan.^2\,\varphi \times 100}{2} - \frac{h_2^2 \times tan.^2\,\varphi \times 100}{2}$$

Where h_1 = vertical depth of actual and assumed fill of earth = 56.625 feet.

Where h_2 = depth of assumed fill above actual fill = 18.125 feet, and P_3 = the horizontal component of the actual fill, with 29 feet of

water in section of reservoir upon one side of division wall; substituting values and working,

$$P_3 = \frac{56.625^2 \times 0.466^2 \times 100}{2} - \frac{18.125^2 \times 0.466^2 \times 100}{2} = 31\,247.35$$

pounds.

The point of application of P_3, above base of wall will be $Y_3 = \frac{2}{3} \times \frac{h_1^2 + (h_1 h_2) + h^2_2}{h_1 + h_2}$ = vertical distance from surface of actual and assumed fill to point of application of P_3; or,

$$Y_3 = \frac{2}{3} \times \frac{56.625 + (56.625 \times 18.125) + 18.125^2}{56.625 + 18.125} = 40.68.$$

and 56.625 feet $- Y_3 = 56.625 - 40.68 = 15.945$ feet.

The horizontal component of the water pressure is 26281.25 pounds, and the point of application $67.5 - \left(\frac{2}{3} \times 29\right) = 48.17$ feet above base of wall, and the point where the combined pressures meet,

$$x_0 = \frac{P_0\ Y_0 + P_3\ Y_3}{P_0 + P_3}$$

Where P_0 is taken as the horizontal component of water pressure.

" P_3 as the horizontal component of earth pressure.

" Y_0 = the vertical distance from base of wall to point of application of horizontal component of water pressure, Y_3 = vertical distance from base of wall to point of application of horizontal component of earth pressure, and x_0 as the vertical distance from base of wall where the combined components meet; or,

$$x_0 = \frac{(26281.\ 25 \times 48.17) + (31247.35 + 15.945)}{26281.25 + 31247.35} = 30.66 \text{ feet.}$$

The counter-pressure of the fill on the opposite side of wall will be

$$P_3 = \frac{38.5^2 \times 0.466^2 \times 100}{2} = 16093.5 \text{ pounds,}$$

and the point of application above base $= \frac{38.5}{3} = 12.83$ feet; and the unbalanced thrust of the components of the earthfill, and water pressure upon side of wall next to the full basin becomes,

$$P = 26281.25 + 31247.35 - \frac{16093.5 \times 12.83}{30.66} = 50794.1 \text{ pounds.}$$

The weight of the wall at center per unit of length is 194 400 pounds, and the weight of the water and earth prism sustained by the batter on one side is 24 634.3 pounds, and of the prism of earth fill on the opposite side 10 587.5 pounds, and total weight on base = 229 621.8 pounds.

The center of gravity of the section of wall combined with the prism of earth and water upon one side, and the prism of earth on the other,

has been taken as 14.33 feet from a vertical intersecting the base of wall upon the water side; and the horizontal distance from center of gravity to center of pressure,

$$x_1 = \frac{50794.1 \times 30.66}{229621.8} = 6.78 \text{ feet},$$

and from center of pressure to extremity of joint, or base,

$$u = 30 - (14.33 + 6.78) = 8.89 \text{ feet},$$

and pressure per square foot at extremity of joint,

$$P_y = \frac{2}{3} \times \frac{229621.8}{8.89} = 17\,219 \text{ pounds},$$

or 119.6 pounds per square inch.

The feature of special interest, however, in the construction of this reservoir, is the main retaining wall; this is an elaborate specimen of rubble masonry and a handsome piece of heavy architecture, although possessing, in the judgment of the writer, several defects when viewed from an engineering standpoint.

The retaining wall or dam, at the deepest part of the ravine which it spans, has a vertical height or depth of 118.66 feet, a base width of 48.75 feet, and a uniform width of top, not including the arches or buttresses, of 15 feet. Of the maximum height of wall, 3.85 feet is represented by the invert under the outfall drain, and the net vertical height of wall, for purposes of calculation, is taken as 114.8 feet, with a base width of 47.5 feet.

The distance from center to center of the projecting buttresses is 68 feet, and the clear span of arch 48 feet.

The wall, not including buttresses or arches, at any point in its height, is of uniform section throughout its length, and calculations of the section at the center of the ravine will apply to any section of the wall of corresponding depth from coping.

At the water face of the wall the maximum depth of fill is 83.66 feet, and at the back of the wall the fill is about 58.43 feet deep, leaving an exposed face of wall subject to water pressure of 35 feet, and an unsupported depth of back of 60.23 feet.

The face of the wall is vertical, and the back has a batter of 4.25 inches to one foot for a height of 53.43 feet from base; and for the remaining 65.228 feet of height, it has a batter of 2.4 inches to 1 foot.

The projecting piers or buttresses which support the arches at back of wall have a batter of 4 inches to the foot for a height of 49.52 feet from base, a batter of 1.3 inches for an additional height of 29.69 feet

to the springing line of the arch, and from this point to top of wall the buttresses are vertical. The construction of the wall, buttresses and arches will be readily understood by reference to Plate XXI, which gives a section of the main wall at the center of the ravine and a partial elevation of the back.

The chambers in the body of the wall shown in the drawing of section are longitudinal in direction, and were calculated to receive and conduct to the outfall drain at the bottom of the wall any leakage from the reservoir through the wall, the surface water from the roadway on the top of the wall and to serve as drains for water from the soil at the extremities of the wall.

In construction these chambers (according to report) were allowed to fill with rubbish and offal from the work, and their functions as drains either limited or wholly destroyed.

In plan the dam is a vertical segmental arch, with the concave side to the water, the chord of which is 840 feet, and the lineal measurement of the four principal segments 925.56 feet. At the southwest end of the wall it joins the effluent pipe chamber of the division wall with a vertical arch of 36 feet radius and 52.5 feet chord upon its soffit, and at the northeast end it joins the paved slope on the side of the hill with a vertical arch of 85 feet radius and 175.5 feet chord, measured upon its soffit.

During January of 1886 the writer's attention was drawn to the discharge of water through the outfall drain under the main wall, which was entirely too large to be accounted for by subterranean drainage; and as the back of the wall was cracked and exuded water at a point near the bottom of the reservoir and nearly over the outfall drain, it was deemed advisable to draw the water from the basin next the wall and examine its condition. Whereupon it was discovered that the wall was cracked upon its face from a point about the usual water line down to and under the concrete floor.

Upon removing the concrete, the earthfill below was found considerably saturated with water; at some points it had settled away from the concrete several inches, and was so soft as to render excavation quite difficult, for the purpose of exploring the crack in the wall. The crack itself extended as deep as the examination was made, and sensibly widened from the top downwards, from which it was inferred that the break was due to vertical settlement of the masonry on its foundation.

By referring to the plan of the reservoir, Plate XIX, the position

of the chamber "A" and tunnel leading from this to the influent chamber "B," at the east end of the division wall, will be noted. The chamber is of rubble, and the influent tunnel leading from it to the influent chamber is of brick masonry. Two 40-inch pipes with flange ends were built in the wall opposite the tunnel at a point 64 feet below the coping, one of which was completed (as shown by dotted lines) through the tunnel to the influent chamber at the east end of division wall. The tunnel rises with an unknown grade from the chamber "B." The chambers "A" and "B" and the influent tunnel were calculated to be impervious.

For several years the reservoir was supplied through the 40-inch influent pipe in the tunnel, until it was discovered, in 1881, that the water stood in what was intended to be a dry chamber ("A") at the same level as the water in the basin, when the influent pipe and tunnel and influent chamber "B" were abandoned, and the delivery of water to the reservoir made through the effluent chamber "C," at the southwest end of the division wall.

Subsequent investigation showed that the influent pipe had broken at the first joint inside the main dam or retaining wall, the barrel of the pipe separating from the flange, and leaving a space all around of about $\frac{3}{4}$ inch through which the water passed into the tunnel and the two chambers.

Recent investigation by the writer shows the arch at the bottom of chamber "A" and the arch of the influent pipe tunnel to be cracked and the joints open, through which water has probably passed under pressure of 18 to 25 pounds per square inch into the earthfill over the tunnel and around the chamber.

This infiltration through the fill has diminished the power of the earth to support the concrete floor, and as a result many cracks have occurred in the latter through which the water has passed downwards from the basin into the fill below.

In addition to the vertical crack in the principal wall, the concrete floor and the fill below were found to have sunk around the chamber "A," and from the quantity of mud in the chamber and the damaged condition of its arch, it was believed that a portion of the earthfill had found its way into the chamber and tunnel.

In the design of the reservoir it was assumed that in its use no water other than subterranean drainage would be found under the floor of the

basins, and the ramifying sewers and drains were supposed to be adequate to the interception and removal of this water, and the prevention of any serious saturation of the earth fill below the concrete.

In brief, it was expected that none of the water pumped into the basins would ever pass below the concrete floor.

No measurements were made of the flow through the outfall drain previous to emptying the section of reservoir next the main wall, owing to the difficulty and danger at that time of working in the sewer; but from comparison of the pumpage before and after the basin was abandoned, it is probable that the average loss of water was upwards of 3 000 000 gallons per diem.

Part of this loss was through the cracks in the back of the masonry wall, but the larger portion passed through the crack in the face of the wall into the drainage chambers, and into the outfall drain, and through the openings in the floor of the basin through the fill into the intercepting drains, and finally into the same outfall drain through the base of the wall.

The use of the basin has been abandoned since January, 1886.

Returning to a consideration of the principal wall or dam, the base loads on the foundation and the stability have been calculated as follows, estimating the weight of the masonry at 144 pounds per cubic foot:

Volume of masonry in wall per unit of length, exclusive of arch, cornice and belt courses and projecting buttresses, 3587.5 cubic feet.

Volume of masonry in arch, cornice and belt courses, pilasters and projecting buttresses per unit of length, 251.13 cubic feet.

Average load per square foot of base =

$$Wo = \frac{3587.5 + 251.13}{47.5} \times 144 = 11637.1 \text{ pounds.}$$

Estimating the load upon foundation for a length of wall corresponding to the base length of buttress (20 feet), and assuming the weight of arches to be entirely sustained by the buttresses, and that such section of wall is capable of independent vertical motion, then the volume of masonry would be 17076.97 cubic feet, or 853.85 cubic feet per unit of length of section, and the average pressure per square foot of base,

$$Wo = \frac{853.85 + 3587.5}{47.5} \times 144 = 13464.3 \text{ pounds;}$$

and similarly for a section of the wall if the projections on the back were removed,

$$= Wo = \frac{3587.5 \times 144}{47.5} = 10875.8 \text{ pounds per square foot,}$$

These average loads are not excessive, and excepting water of the subterranean drainage or from the basin of the reservoir has passed downwards below the base of the wall and softened the marl strata, or the clay seams between the underlying strata of lime-stone, no settlement sufficient to break the wall should have occurred.

From a comparison of the present pumpage into the reservoir per diem, with the pumpage of the past four or five years, it would appear that the crack in the wall and its corresponding settlement, and the damage to the floor of the basin were not of recent occurrence.

Several years ago (perhaps at the time when the breaks in the wall and concrete floor occurred) the consumption of water, as exhibited by the pumpage into this reservoir, showed a considerable and unwarranted increase over previous and similar periods of time; to correct which and keep down the average consumption per capita, the water department attempted an increase of population of over six per cent. per annum, when the natural increase per annum is 1.5 per cent.

(The increase of population perpetrated by the water department must not be taken in a literal sense).

In estimating the strains which may be applied to the wall from the water side, we have the horizontal pressure due the earthfill below the basin in its normal condition, combined with the horizontal pressure of the water in the basin.

Should this earthfill become saturated with the leakage from the basin, and the fill become a mixture of earth and water, with an angle of repose of 0 or nearly 0, and a specific gravity of 1.6, then the horizontal component of the pressure of the earthfill below the basin, combined with the water above the fill, would be greatly increased, as is well known. Of course, in the design of the basin, no such condition as a thorough saturation of the earthfill was contemplated; and in fact (as it was supposed), ample provision was made to conduct away water from whatever source which should find its way into or through the fill.

But sewers and drains sometimes become clogged and fail to discharge their functions as conduits; and since these drains are deep in the ground, difficult of access and seldom explored, is it not possible that a work of disintegration might go on through years of time, which eventually would diminish the carrying capacity of the subterranean sewers, or close them entirely; in which event not alone the water of leakage from the basin, but the subterranean drainage, would accumu-

late in the earthfill above the wall, and subject it to pressures far beyond the expectation of its constructors.

In the construction of the reservoir it was believed that the bottom could be made water-tight, and that the drains would be required to conduct away only the subterranean and such surface drainage as was calculated to pass downwards through the channels for the purpose constructed in the wall; but in this respect the work has signally failed, and for some years the fill has been open to saturation by the leakage from the basin.

To subject this wall, however, to a strain far in excess of that contemplated by its builders, it is not necessary to saturate the entire fill or any large portion of it; if the earth next the wall be gradually softened by leakage through the joint made between the concrete and the wall, until this extends down or nearly down to the footing courses, a pressure may be applied equal to or greater than that due a column of water of same height.

The earth support at the back of the wall, as shown in the section, Plate XXI, is fill also; but although loosely deposited (as the writer is informed), is probably, from its free exposure to the action of the elements, equal in compactness and natural stability to the fill at the face of the wall, which is supposed to have been rolled and tamped more or less during its introduction.

In the following calculations the general assumptions are made:

First.—That the pressures upon the wall, from materials in front or behind it, are developed in horizontal planes.

Second.—That the mortar has no cohesive effect, and the horizontal component of any vertical load of earth or water is limited to the joint in the masonry for which such load is estimated.

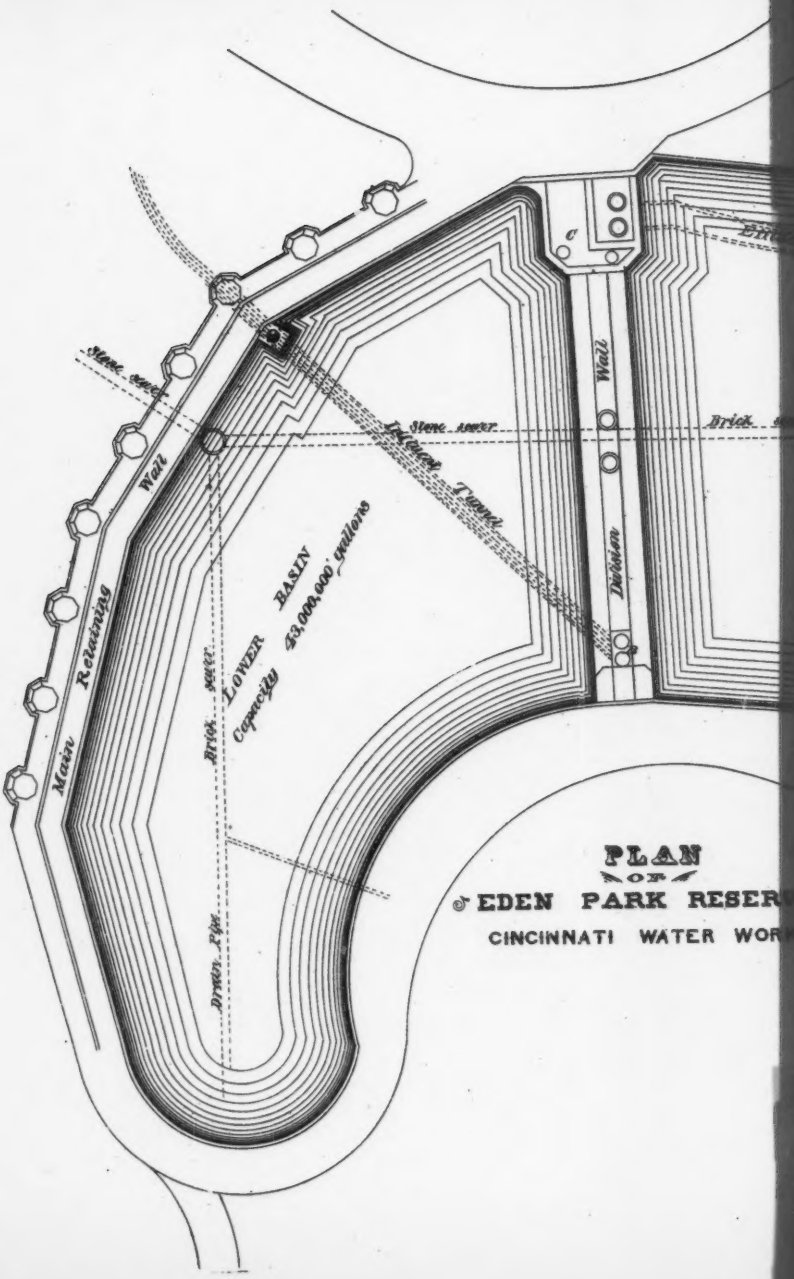
Third.—That the horizontal thrusts of the earth or water have no effect either to increase or diminish the vertical loads of the wall, and that the pressure or load on any joint cannot exceed, nor be less than, the weight of masonry above that joint.

The data for quantities of material have been estimated from measurements on the original drawings.

Section of wall between buttresses or counterforts.

Section of wall opposite water, 35 feet deep; top width, 15 feet; bottom width, 22 feet; volume per unit of length, 647.5 cubic feet; weight on joint, 35 feet from top = 93 240 pounds.

Section of wall above earthfill at back, 60.23 feet from top; top width, 15 feet; bottom width, 27.25 feet; volume per unit of length =



PLAN
OF
EDEN PARK RESER
CINCINNATI WATER WOR

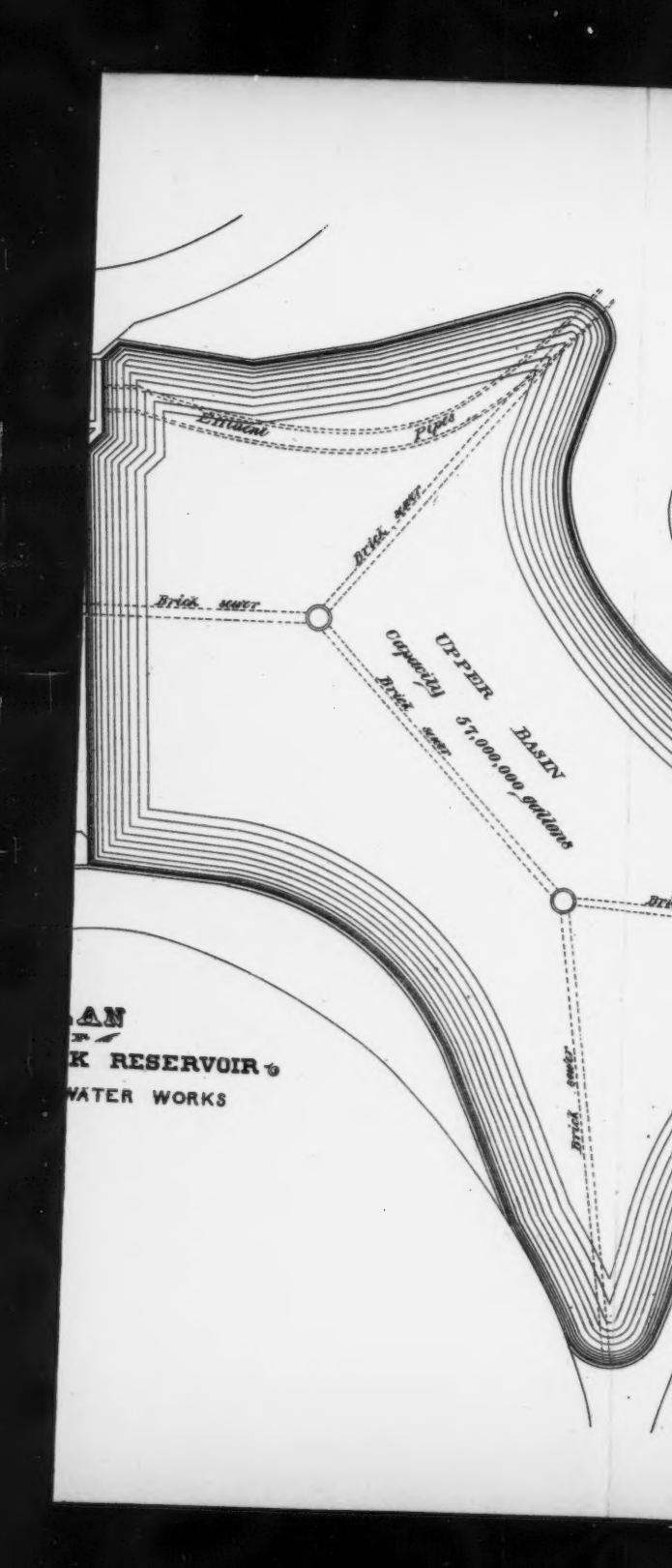
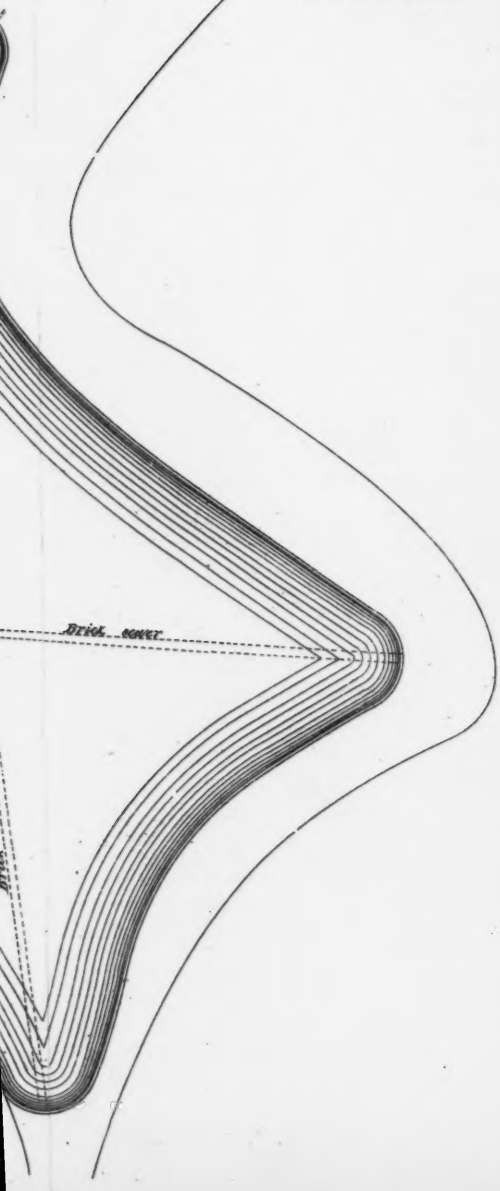
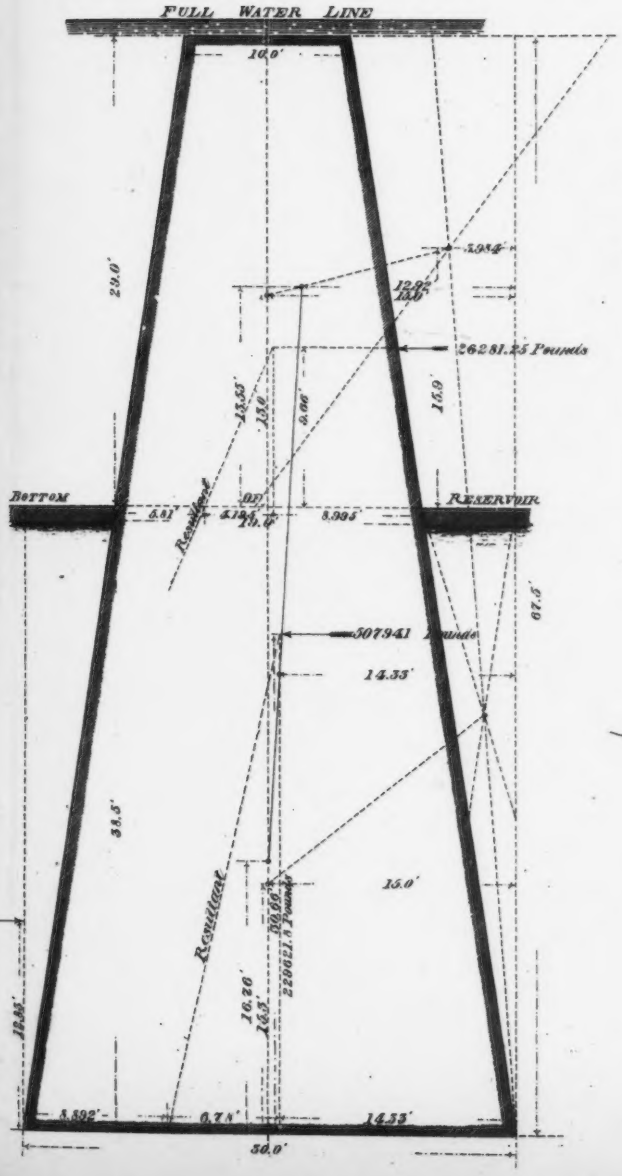

Brick sewer
Brick sewer
Brick sewer
UPPER BASIN
capacity 57,000,000 gallons
Brick sewer
Brick sewer
Brick sewer
AN
K RESERVOIR
WATER WORKS

PLATE XIX
TRANS. AM. SOC. CIV. ENGRS.
VOL XVI Nº 360
HILL ON
A MASONRY DAM



SECTION OF DIVISION WALL

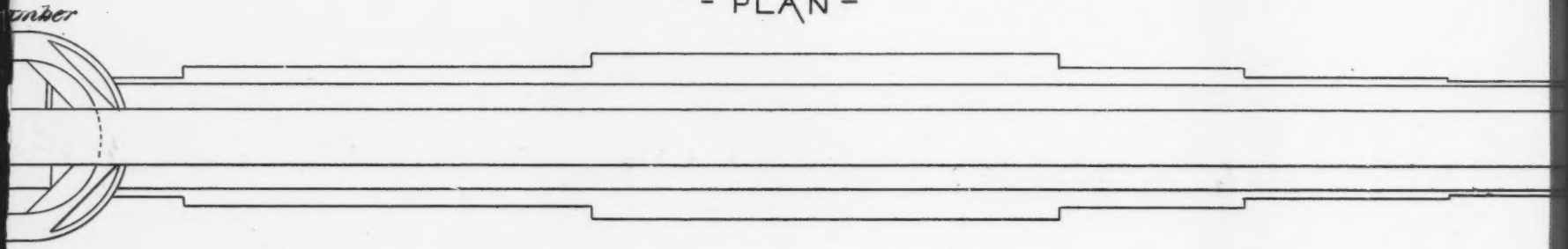


Effluent Chan

DIVISION WALL - EDEN RESERVOI

CINCINNATI WATER WORKS

- PLAN -



- ELEVATION -

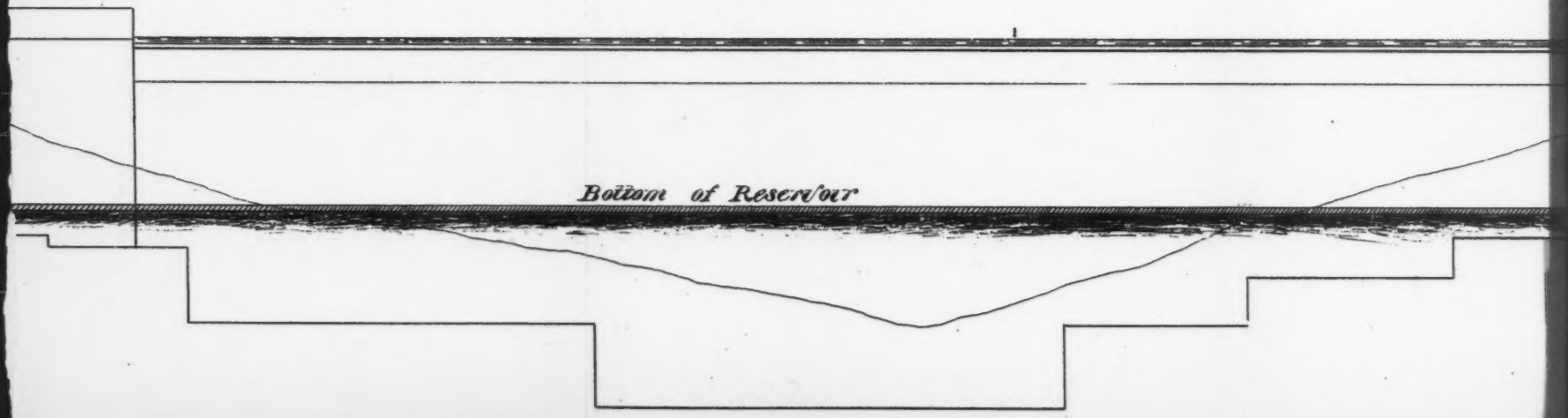
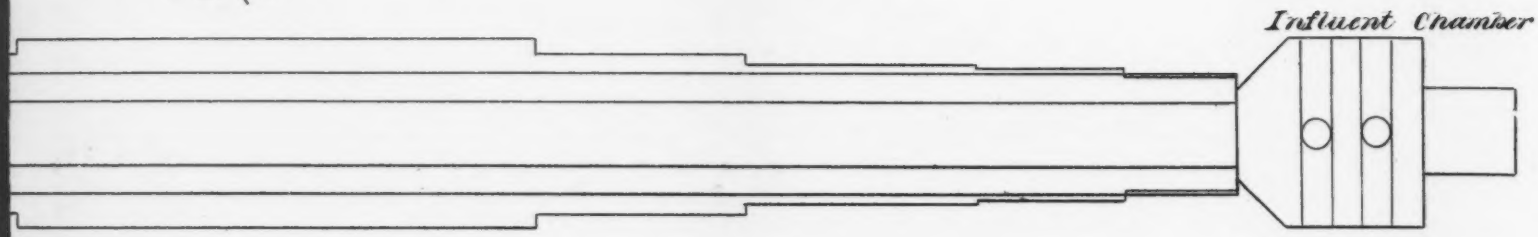


PLATE XX
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 360
HILL ON
A MASONRY DAM

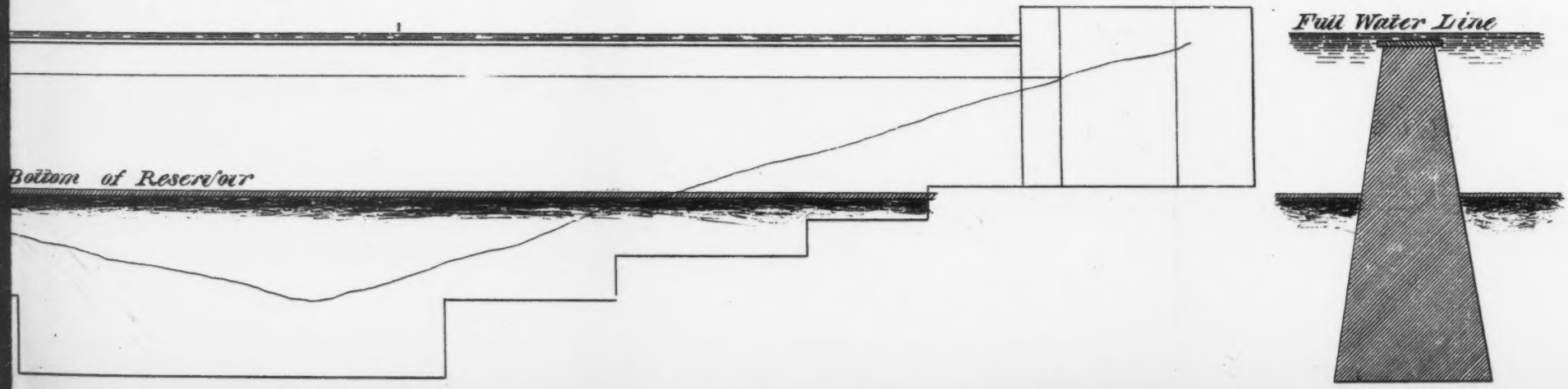
WALL - EDEN RESERVOIR

CINCINNATI WATER WORKS

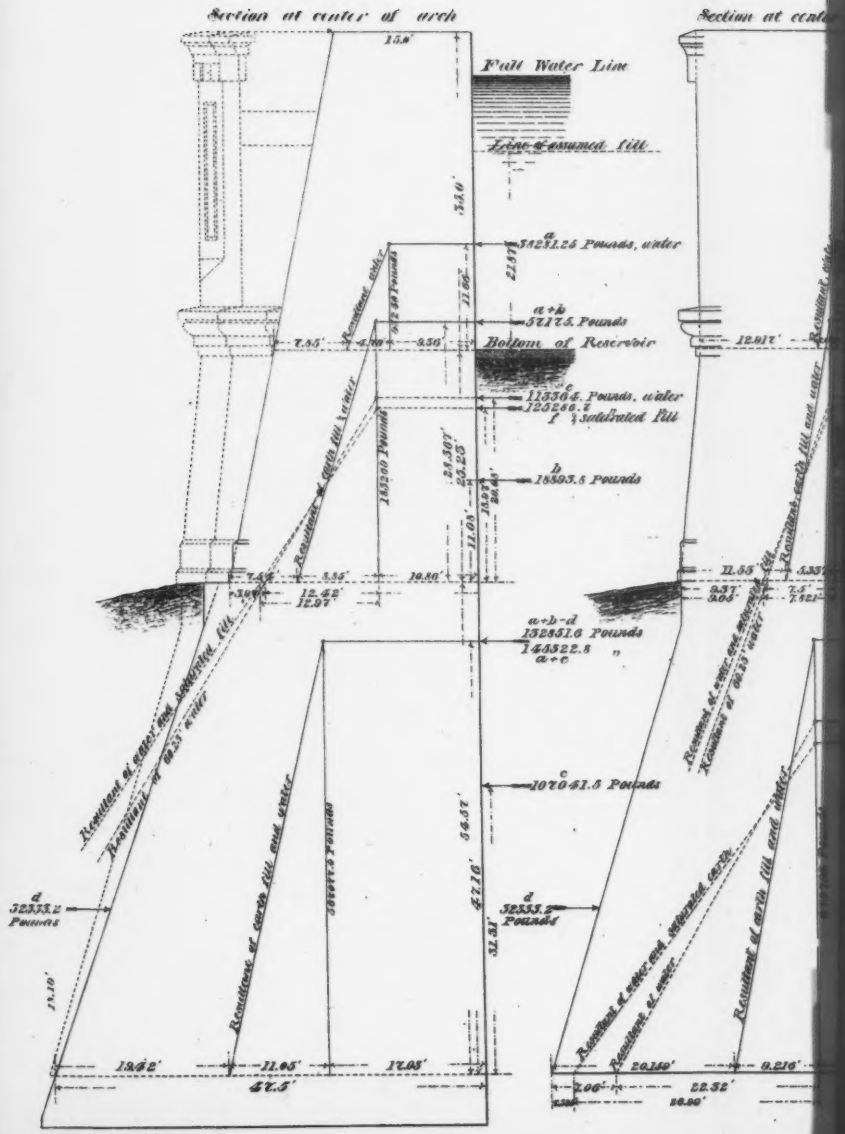
- PLAN -



- ELEVATION -



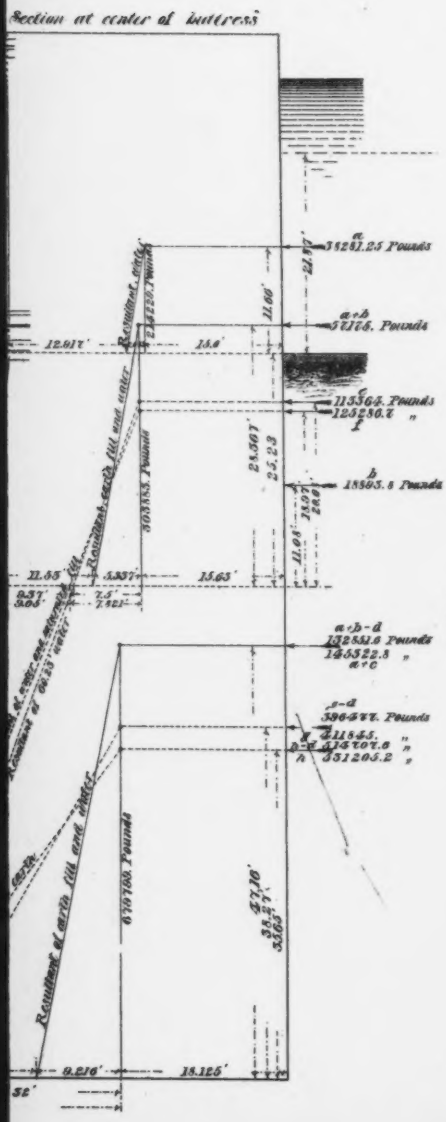
MAIN RETAINING WALL

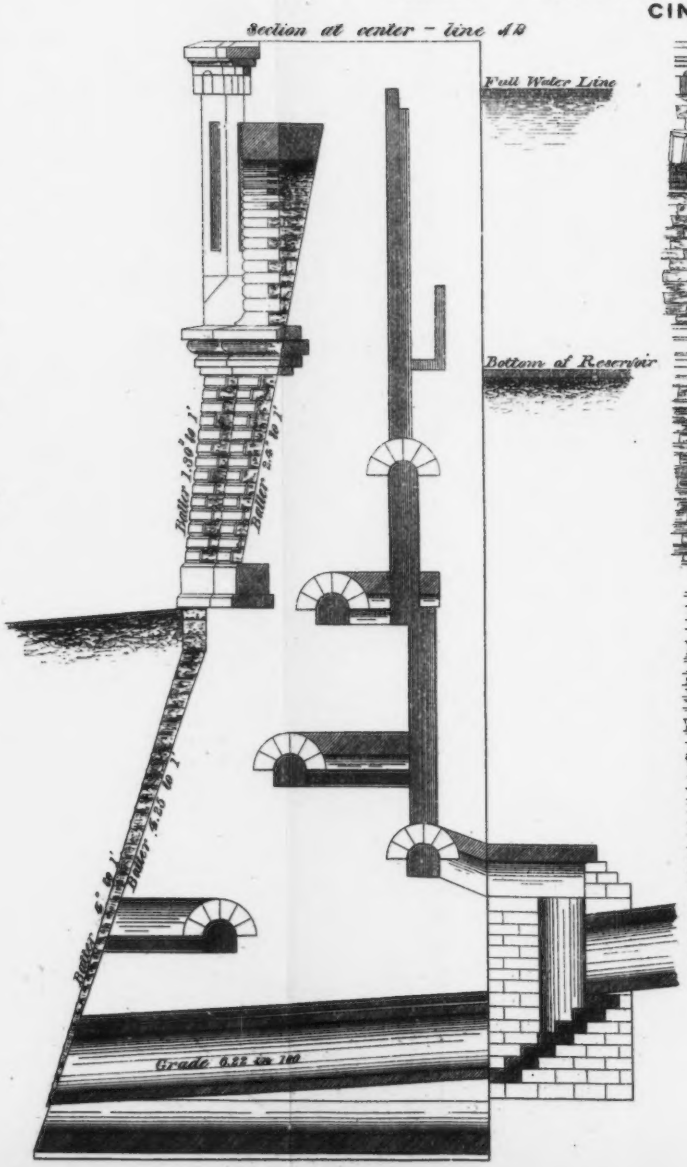


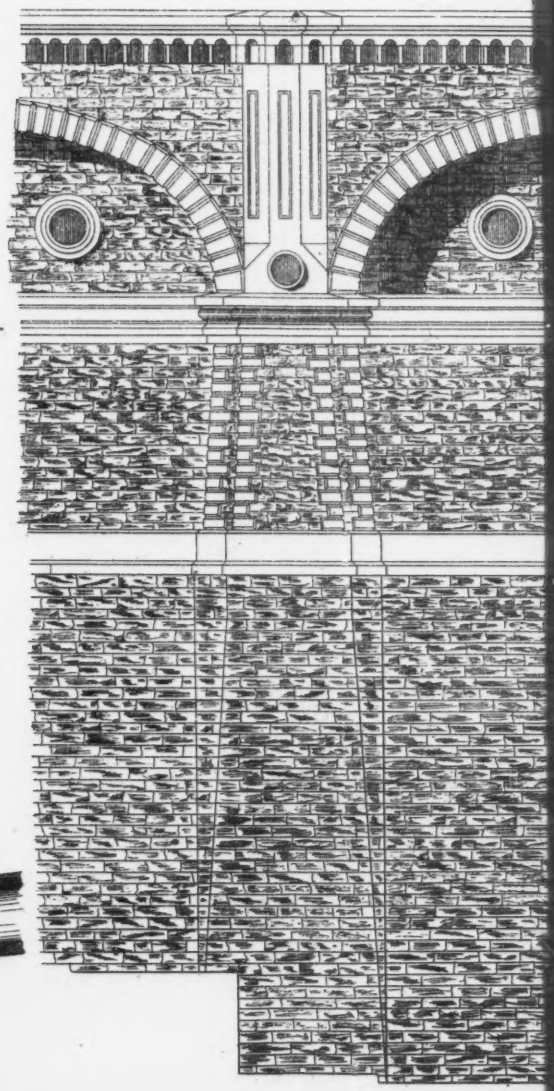
ALL

MAIN RETAINING WALL - EDEN R

CINCINNATI WATER WORKS



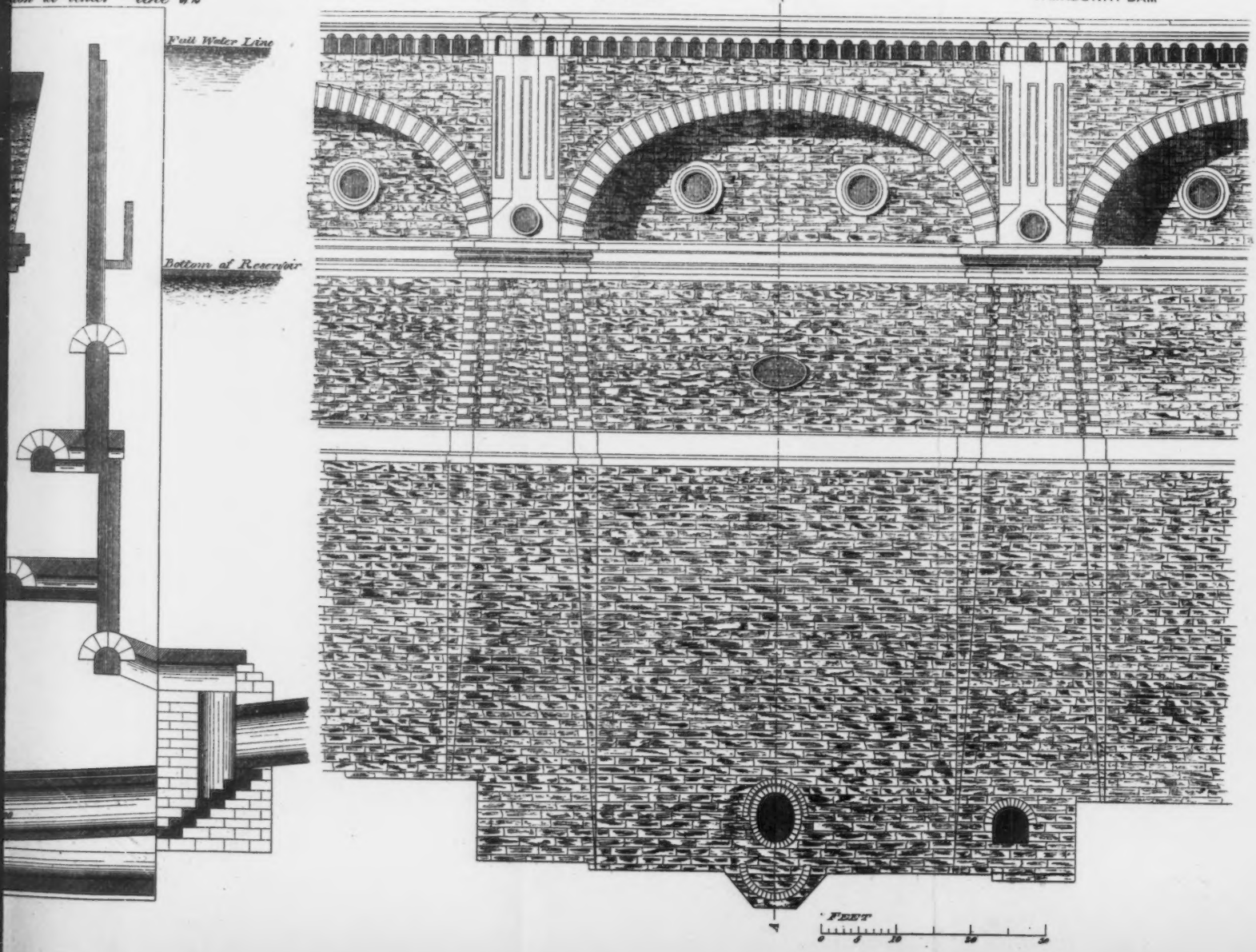

Section at center - line AB
Full Water Line
Bottom of Reservoir
Batter 1.36 to 1'
Batter 2.4 to 1'
Batter 4.15 to 1'
Batter 6 to 1'
Grade 0.22 in 100



MAIN RETAINING WALL - EDEN RESERVOIR

CINCINNATI WATER WORKS

PLATE XXI
TRANS. AM. SOC. CIV. ENGRS
VOL. XVI Nº 360
HILL ON
A MASONRY DAM



1 272.7 cubic feet, and weight on joint 60.23 feet from top = 183 269 pounds

Entire section of wall at depth from top, 114.8 feet; top width, 15 feet; bottom width, or base width, 47.5 feet; volume per unit of length, 3 587.5 cubic feet; and weight on foundation, 516 600 pounds; to which must be added the weight of the prism of earth below the surface of fill at back of wall, supported by the batter $= \frac{54.57 \times 18.5 \times 100}{2} = 50\,477$ pounds, and the total weight on foundation is, $W = 567\,077$ pounds.

In estimating the quantities and weights for the section of wall opposite and including a buttress, it is assumed that the weight of an arch as shown in the elevation of back of wall is borne by the adjacent buttresses and section of wall from which the buttresses project.

Volume of masonry above joint, 35 feet below top of wall, per unit of length, 1 487.7 cubic feet; weight on joint same depth, 214 229 pounds.

Volume of masonry above joint, 60.23 feet below top of wall, per unit of length, 2 110.3 cubic feet; weight on joint at same depth, 303 883.2 pounds.

Volume of masonry above foundation, 114.8 feet below top of wall, per unit of length, 4 441.35 cubic feet; weight on foundation, 639 554.4 pounds; to which must be added the weight of the prism of earth below the surface of fill at back of wall, supported by the batter $= \frac{54.57 \times 14.75 \times 100}{2} = 40\,245.$ pounds; and the total weight on foundation becomes 679 799 pounds.

The centers of gravity, or rather the horizontal ordinates from face of wall to the centers of gravity of the three horizontal sections, taken for purposes of calculations, are as follows:

For section of wall between buttresses; center of gravity from face of wall for horizontal section above joint, taken 35 feet from top, 9.36 feet.

For horizontal section above joint, taken 60.23 feet from top, 10.86 feet.

For section of wall from foundation, 114.8 feet from top, 17.03 feet.

For wall opposite and including buttresses:

Center of gravity from face of wall for horizontal section, taken 35 feet from top of wall, 15 feet.

Center of gravity from face of wall for horizontal section, taken 60.23 feet from top, 15.63 feet.

Center of gravity of entire section of wall from foundation, 114.8 feet from top, 18.125 feet.

Estimating the stability or strength of the wall after the method proposed by M. Debauve, the pressure upon the joint at back of wall, at depth of 35 feet from the top, has been calculated as follows:

Horizontal distance from center of gravity of section of wall to center of pressure,

$$x_1 = \frac{38\,281.25 \times 35}{93\,240 \times 3} = 4.79 \text{ feet};$$

and center of pressure from edge of joint, of section of wall between buttresses,

$u = 22 - (9.36 + 4.79) = 7.85$ feet; then, the pressure per square foot at extremity of joint or back of wall =

$$P_y = 2\Big[(2 \times 22) - (3 \times 7.85)\Big] \times \frac{93\,240}{22^2} = 7\,879 \text{ pounds},$$

or 54.72 pounds per square inch.

The limiting pressure, according to M. Debauve, is 92.5 pounds per square inch.

In estimating P for the section of masonry above a joint 60.23 feet from top of wall, or at the joint where the outside earthfill terminates, we have to consider the pressures developed by the inside fill above this point, and the depth of water, which may be 35 feet, and the depth of earth fill to the joint, 25.23 feet.

The angle of repose of the earthfill has been taken at 40 degrees, and the angle of the plane of fracture which the earth wedge will make with the face of wall $= \frac{90-40}{2} = 25$ degrees.

It will be convenient to estimate the horizontal component and its point of application first for the earthfill, and then combine the component so found with that of the water in the basin.

Let the specific gravity of the earthfill be taken at 1.60 water, then will a column or depth of earth of $\frac{35}{1.6} = 21.87$ feet produce the same base load as the depth of water.

The actual depth of the earthfill to the joint under consideration is 25.23 feet, and the whole depth, for the purpose of calculating the horizontal pressure, is

$$25.23 + 21.87 = 47.10 \text{ feet}.$$

Let H = depth of wall to joint, $H - h = h_1 = 47.10$, $h_2 = 21.87$ and $h_1 - h_2 = h_3 = 25.23$ feet.

Let $P_1 = \frac{{h_1}^2 \text{ tang. } {}^2\varphi \times 100}{2} = \frac{47.1^2 \times 0.466^2 \times 100}{2} = 24\,087.05$ pounds,

and

$$P_2 = \frac{{h_2}^2 \text{ tang. } {}^2\varphi \times 100}{2} = \frac{21.87^2 \times 0.466^2 \times 100}{2} = 5\,193.25 \text{ pounds};$$

then the horizontal pressure upon the wall due to the earthfill and superimposed depth of water will be,

$$P_3 = P_1 - P_2 = 18\,893.8 \text{ pounds}.$$

The point of application of P_1 will be from the surface of the actual and assumed fill, $y_1 = \frac{2}{3} h_1$ and the point of application of the pressure P_2 will be from same surface $y_2 = \frac{2}{3} h_2$ and the point of application of the presssure,

$$P_3 \text{ is } y_3 = \frac{2}{3} \times \frac{h_1^2 + (h_1\, h_2) + h_2^2}{h^1 + h_2} =$$

$$\frac{2}{3} \times \frac{47.1^2 + (47.1 \times 21.87) + 21.87^2}{47.1 + 21.87} = 36.02 \text{ feet},$$

and distance from joint to point of application of pressure, $47.1 - 36.02 = 11.08$ feet, and from top wall 49.15 feet.

The horizontal thrust of the water is 38 281.25 pounds, and is applied at a depth of $\frac{2}{3} \times 35 = 23.33$ feet from top of wall.

Taking moments around the upper edge of wall coincident with its face, where the horizontal component of the water pressure, lever arm upon which such component acts, and the resistance of wall $= o$; we have, as the combined horizontal pressure, 57 175.05 pounds, and the point above the joint in the masonry (60.23 feet from top) where the components meet, as

$$x_0 = 60.23 - \frac{(38\,281.25 \times 23.33) + (18\,893.8 \times 49.15)}{38\,281.25 + 18\,893.8} = 28.367 \text{ feet}.$$

The horizontal distance from the center of gravity to the center of pressure, for wall between buttresses will be,

$$x_1 = \frac{57\,175.05 \times 28.367}{183\,269} = 8.85 \text{ feet},$$

and horizontal distance along the joint from center of pressure to edge of joint is,

$$u = 27.25 - (10.86 + 8.85) = 7.54 \text{ feet},$$

and pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{7.54} = 16\,204 \text{ pounds},$$

or 112.5 pounds per square inch.

Assuming a water pressure for the same depth of wall, which is a condition that may at some future day subsist, we have:

Horizontal component of head of water, 60.23 feet;

$$P = \frac{60.23^2 \times 62.5}{2} = 113\,364.15 \text{ pounds},$$

and the horizontal distance from center of gravity of the section of wall to the center of pressure will be,

$$x_1 = \frac{113\,364.15 \times 60.23}{183\,269 \times 3} = 12.42 \text{ feet},$$

and $u = 27.25 - (10.86 + 12.42) = 3.97$ feet, and pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{3.97} = 30\,775.6 \text{ pounds},$$

or 213.7 pounds per square inch.

If, however, the water is permitted to leak through the concrete bottom of the basin, it will or may produce a saturated earth, the specific gravity of which would be 1.6 water, and the angle of repose $\varphi = o$, or nearly o; in which event the horizontal component of pressure would be, taking angle (φ) of repose at o,

$$P = \frac{47.1^2 \times 100}{2} - \frac{21.87^2 \times 100}{2} = 87\,005.5 \text{ pounds},$$

and the vertical distance above the joint where such pressure would be applied, as in the case of the actual and assumed earthfill = 11.08 feet, and from top of dam 49.15 feet; and as before for the earthfill, taking moments around the upper inner edge of wall, to determine the point of application above joint, of the combined moments of water pressure and pressure of saturated earth, we have

$$x_o = 60.23 - \frac{(38\,281.25 \times 23.33) + (87\,005.5 \times 49.15)}{38\,281.25 + 87\,005.5} = 18.97 \text{ feet}.$$

The horizontal distance from the center of gravity to center of pressure will be,

$$x_1 = \frac{125\,286.75 \times 18.97}{183\,269} = 12.97 \text{ feet},$$

and from edge of joint to center of pressure,

$$u = 27.25 - (10.86 + 12.97) = 3.42 \text{ feet},$$

and the pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{3.42} = 35\,724.9 \text{ pounds},$$

or 248.0 pounds per square inch, or 2.68 times the limit of pressure assigned to structures of rubble masonry by MM. Debauve, Krantz, Sazelly and Delocre.

Upon comparison of the weight of the section of wall above the joint and the horizontal component of the pressure, it will be obvious that with a coefficient of friction of less than 0.70, the wall would be on the point of sliding outwards.

It may be urged that the pressures calculated for the 60.23 feet depth of water, and the same depth of water and saturated earth, will never subsist; and the calculations exhibit a weakness of wall upon impossible conditions. But, are these conditions impossible? The experience of less than eight years with this reservoir has shown the bottom to have

broken and permitted the passage of water to the earthfill below; and is it possible to make such joints of the concrete with the masonry of the wall that water will not pass below the floor of the basin? Or, if such joints be made, is there an assurance that they will always remain tight, with the concrete resting upon an earthfill subject to indefinite settlement? Excepting this result can be attained, or excepting the reservoir and its adjuncts be subject to better management than has prevailed in the past, the assumed conditions may gradually develop during years of service and the fact of their existence be not known.

This structure cost about $1 700 000, of which the main retaining wall alone cost nearly half, and was not built for a generation or several of them, but presumably for all time.

Masonry dams said to have been built upon better foundations, and known to have had a higher factor of safety than this, have yielded after many years of service; and what immunity from the disintegrating forces in structures of a similar character does this work possess?

Estimating the pressure at the outer edge of wall at its base, we have $114.8 - 35 = 79.8 = h_2 =$ depth of earthfill; and taking the angle of the plane of fracture of the earth wedge with the vertical face of wall $= \varphi = 25°$ the horizontal component of the pressure of the fill against the wall will be,

$$\text{where } h_1 = 79.8 + 21.87 = 101.67 \text{ feet,}$$

and $h_2 = 21.87$ as before for the depth of the assumed additional fill, (the base load per unit of area of which = the base load for a column of water 35 feet high or deep).

$$P_1 = \frac{101.67^2 \times 0.466^2 \times 100}{2} - \frac{21.87^2 \times 0.466^2 \times 100}{2} = 107\,041.53$$

pounds.

$$Y_3 = \frac{2}{3} \times \frac{101.67^2 + (101.67 \times 21.87) + 21.87^2}{101.67 + 21.87} + 13.13 = 83.49 \text{ feet,}$$

and the point above foundation where the combined components of the water pressure and the earth pressure meet,

$$x_0 = 114.8 - \frac{(38\,281.25 \times 23.33) + (107\,041.53 \times 83.49)}{38\,281.25 + 107\,041.53} = 47.16 \text{ feet.}$$

But the thrust of the earthfill, and the depth of water above it at the face of the wall, is partially counterbalanced by the thrust in the opposite direction of the fill at the back of wall.

The depth of this fill from base of wall has been given as 54.57 feet, and taking same angle of natural repose as for the fill at face of wall, and assuming the wedge of earth between a vertical cutting, the base at

the edge and the back of wall exerts no thrust, we have as the horizontal component of the earth pressure,

$$P = \frac{54.57^2 \times 0.466^2 \times 100}{2} = 32\,333.2 \text{ pounds.}$$

The point of application of this pressure is $\frac{54.57}{3} = 18.19$ feet from base, from which is obtained the unbalanced thrust of the horizontal component of the pressure of the earthfill and water at face of wall,

$$P = 38\,281.25 + 107\,041.53 - \frac{32\,333.2 \times 18.19}{47.16} = 132\,851.6 \text{ pounds.}$$

Horizontal distance from center of gravity to center of pressure,

$$x_1 = \frac{132\,851.6 \times 47.16}{567\,077} = 11.05 \text{ feet,}$$

and from edge of joint to center of pressure,

$$u = 47.5 - (17.03 + 11.05) = 19.42 \text{ feet,}$$

and pressure per square foot at edge of joint.

$$P_y = 2\Big[(2 \times 47.5) - (3 \times 19.42)\Big] \times \frac{567\,077}{47.5^2} = 18\,468.46 \text{ pounds,}$$

or 128.25 pounds per square inch.

The pressures at the extremity of the several joints respectively 35 feet from top of wall, 60.23 feet from top, and at base of wall 114.8 feet from top, for the wall opposite and including the buttresses and arch, under the same conditions of loading as taken for the section of wall between buttresses, are tabulated below,

PRESSURES AT EXTREMITY OF JOINT.

(*Wall including Buttress.*)

Vertical depth from top of wall to joint	35	60.23	114.8 feet.
Water pressure, pounds per square foot	10 118.7	21 621	64 192.6 pounds.
Water pressure, pounds per square inch	70.27	150.14	445.78 "
Water pressure and pressure of earthfill, pounds per square foot	.....	17 491.55	20 803.5
Water pressure and pressure of earthfill, pounds per square inch	.....	121.47	144.47
Water pressure and pressure of saturated earthfill, at water side of wall, pounds per square foot	.....	22 385.3	190 021.0
Water pressure and pressure of saturated earthfill, at water side of wall, pounds per square inch	.....	155.45	1319.6

In the calculation of the strains in the wall for its full height the center resistance of the earth at the back has been taken as the passive force due the pressure of the fill against the batter; but if we assume the wall to be in motion, as it would be under the condition of a saturated fill under the basin, with the basin carrying its usual depth of water, then the resistance to motion would be, not the horizontal component, but the abutting power, or, "the greatest horizontal pressure consistent with the stability of the earth."*

* Rankine's Applied Mechanics, pp. 220-221.

This, according to Rankine, is,

$$R = \frac{w\,x^2}{2} \times \frac{1 + \sin.\,\varphi}{1 - \sin.\,\varphi}$$

Let w = weight of the earth per cubic foot, x = vertical depth or height of earthfill or bank, φ = angle of natural repose of the material in the bank, and, R = the greatest horizontal resistance which the fill is capable of opposing to the motion of the wall; then,

$$R = \frac{100 \times 54.57^2}{2} \times \frac{1 + 0.6428}{1 - 0.6428} = 684\,913.6 \text{ pounds.}$$

The center of resistance of the earthfill is $\frac{54.57}{3} = 18.19$ feet, above base of wall, and the total resistance to the sliding of the wall upon its base, becomes

$$R_1 = 684\,913.6 + (679\,799. \times 0.60) = 1\,092\,793 \text{ pounds.}$$

This fill upon the outside or at the back of the main wall is not permanent; was made upon private property; and, excepting the ownership or control of the land below the retaining wall, is vested by legal process in the city; is subject to removal, and such support as it may now offer will then disappear.

It is not likely that the city will either purchase or lease the ground upon which the fill was made until compelled to do so by stern necessity.

Earlier in this paper, which has been expanded perhaps more than the subject warrants, the writer suggested that the main retaining wall was defective in an engineering point of view; and the resultant pressures at the extremities of different joints under different conditions of loading have been worked out, to support the writer's opinion that the design of the wall is defective.

Although the wall was not constructed to meet the conditions of water pressure for its entire heighth, or of the greater pressure due a saturated earthfill, it should have been so constructed, because such conditions are not only possible, but have partially subsisted for the past five years. I remember reading some years since, in a work on masonry dams, a suggestion that such walls should always be constructed to meet a water pressure for the full height, and I have no knowledge of any other masonry dam which is constructed as a retaining wall for earth pressure for a portion of its height, and as a dam for water pressure for the remainder.

Assuming the wall should have been built upon the principles em-

ployed in the construction of masonry dams, and comparing its section with the profiles of Rankine, Graeff and Mongolfier, we find it lacking in base width and subject to resultant pressures greatly in excess of those taken as a safe limit by the older constructors. It will of course be urged that the wall can never be subjected to water pressure for its full height, because the sewer through the base will be large enough to conduct away all water which may filter through the fill under the basin. But in a structure of this character, the destruction of which would be fraught with disaster not alone to the residents below it, but to the Water Department, which depends upon this reservoir as a distributor of more than three-fourths the total consumption of the city and its suburbs, is it good engineering to base the safety of such an important structure upon so many assumptions, *i. e.*, that the sewer will never become clogged or choked and fail to discharge the drainage delivered to it; that water will never accumulate under the concrete floor above the wall, and subject it to pressures beyond those provided for in its construction; and that a saturation of the earthfill under the water basin is not likely to occur?

The volume of masonry per unit of length is now quite 75 per cent. of what would have been required to construct a dam after any of several well known and acceptable profiles, and it seems to the writer if some one of these had been adopted, the present damage to the wall would not have occurred, and the break in the concrete bottom of the basin, and the saturation of the fill below, would have been of little consequence as affecting the stability of the wall. As it is, the future success of the main wall depends entirely upon the ability of the Water Department to render the bottom water-tight, or to construct vertical drains at the face of the wall, which will convey the leakage quickly into the outfall sewer and prevent its accumulation in the earthfill.

But the latter arrangement is a poor expedient, and if adopted may result in a daily loss of several million gallons of water pumped against a total head of 250 feet from the Ohio River into the reservoir.

DISCUSSION.

William E. Merrill, M. Am. Soc. C. E.—In the spring of 1886 I was employed by the Board of Public Works, on the recommendation of Mr. John W. Hill, M. Am. Soc. C. E., Consulting Engineer to the Board, to examine the Eden Park Reservoir in the City of Cincinnati,

and report on the best method of repairing it. The main retaining wall was cracked, and the concrete floor had a break in it, besides several long cracks.

Soon afterwards the Board of Public Works was legislated out of office, and their place was taken by the Board of Public Affairs. My appointment was continued under Mr. Arthur Moore, Mr. Hill's successor, and my final report was submitted in August, 1886.

It was my conclusion that the main retaining wall was safe as long as the pressure of the filling underneath the concrete floor remained an earth pressure, and did not become a mud pressure. As the latter condition was not improbable, it seemed necessary to provide against it, either by increasing the cross section of the retaining wall until it became strong enough to sustain the maximum possible pressure of mud or water, or else to devise methods of preventing saturation by and the creation of mud.

As the first plan would have entailed a very heavy outlay on the part of the city, I decided to recommend the second. This plan, which was adopted, consisted in digging a trench along the inner face of the retaining wall down to bed rock; building a drainage culvert with open joints on the floor of the trench; replacing the excavated earth by gravel, broken stone, or other porous material, and finally extending the concrete flooring over the top of the filled-up trench. My theory was that whatever leakage came through the concrete floor into the filling would ultimately find its way into the porous vertical stratum, and thence pass off through the drainage sewers without saturating the fill. As these sewers are easily accessible from outside, any excess of leakage can easily be detected and prevented after emptying the basin. As the water of the Ohio River is usually heavily charged with sediment, the floor of the basin is quickly covered with mud, and all minor cracks in the concrete are soon choked up.

The cracks in the main retaining wall are doubtless due to settlement of the foundations, and there is no other remedy than to fill them up as they occur, which is a comparatively easy matter, as the basin can readily be emptied.

I cannot agree with Mr. Hill that the loss of water through the porous lining next to the main wall will be excessive, as this lining will be compacted by the water pressure above it, and will be covered with the general concrete floor of the basin. It is evident that at first there will be considerable settlement, with probable rupture of the concrete floor; but this condition can easily be detected through the drainage culverts, and the basin can be emptied and the concrete repaired. The same thing might occur several times at increasing intervals, but the cost of repairs would be trifling, and the settlement would ultimately cease. In my judgment this method of treatment is much more rational than to dig down sixty feet and more (for the ravine descends rapidly) and build another wall against the face of the present one.

I would state, in conclusion, that my calculations for the pressure of earth against retaining walls were based on the methods indicated by M. A. Gobin, Chief Engineer of *Ponts et Chaussées*, in an article published in the *Annales des Ponts et Chaussées* in the latter half of 1883. This article demonstrates the unsoundness of Rankine's hypothesis, and as it is in accord with the latest experiments on retaining walls, I took it as my guide. There are several articles on earth pressures in recent numbers of the *Annales* which are worth consulting by members interested in such subjects, but the article of Gobin is the least obscured by excessive mathematics, and is to me the most satisfactory of them all.

JOHN W. HILL, M. Am. Soc. C. E.—The principal objection to Colonel Merrill's method of repair for the damaged east basin of Eden Reservoir, was that it assumed a continuance of the leakage through the concrete floor, which, as a measure of economy (if not safety) in the use of the basin was, if possible, to be avoided.

The water as pumped into the reservoir from the Ohio River carries in suspension the usual amount of clay and silt, which will in time close the small cracks and fissures in the concrete floor; but under a head of 30 feet of water, cracks or open joints between the concrete and the masonry walls would pass the suspended matter quite as freely as water.

At the time of examining the damaged basin, it seemed to me the proper method of repair was to remove the concrete floor over such portions of the earthfill as was saturated with the leakage, and after restoring the loss of fill and rendering the soil as compact and solid as possible, by rolling and hand ramming, to relay the concrete floor; being careful in making the joints between the old and new concrete to avoid leakage, and constructing the joints between the concrete floor and the wall in such a manner that the head of water would press the concrete against rather than force it away from the masonry.

A method of repair which was calculated to prevent the leakage of water through the bottom of the basin, was especially to be desired, since it not only removed the danger of over-pressure on the dam or retaining wall, but saved the Water Department several thousand dollars per annum by avoiding the pumpage of water into the basin to compensate for the leakage.

The formula for abutting power, or resistance to horizontal thrust, of a vertical plane of earth, was taken from Rankine's Applied Mechanics, without any independent investigation of its exact merits; and for information it is regretted that Colonel Merrill did not restate the equation for the active resistance of the earthfill at the back of the retaining wall, from the data given according to the method proposed by M. Gobin.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

361.

(Vol. XV.—June, 1887.)

STEEL: ITS PROPERTIES; ITS USE IN STRUCTURES AND IN HEAVY GUNS.

By WILLIAM METCALF, M. Am. Soc. C. E.

READ MARCH 2D, 1887.

WITH DISCUSSION.

Much of what is written in this paper has been written and published before, yet it seemed better to indulge in some repetitions in order to try and bring together sufficient data on which to base a reasonable argument. A few definitions may not be out of place, as there seems to be some confusion in the use of terms in recent writings.

The word temper has two distinct meanings among steel-makers. Applied to steel not hardened, the temper is said to be mild, medium or high, according to the amount of carbon the steel contains; thus we recognize and use daily in the crucible-steel business fifteen tempers, each so distinct from the other in the fractured ingots, that there is no uncertainty in their selection and separation.

The mean difference in carbon between any two adjacent tempers is .07 per cent. When speaking of the temper of a piece of tempered steel, the final condition of the steel is referred to; that is to say, it is straw-color, orange, light brown, brown, pigeon-wing or blue, as the case may be. If the piece has not had the temper drawn, it is said to be hardened and not tempered.

To temper a piece of steel is to heat it, harden it by quenching, and to draw the temper to the color or degree of softness required.

The recent United States Navy specifications would read better if they said to be annealed, hardened in oil, and to have the temper all drawn out.

Steel-makers call the last operation drawing black.

Annealing steel is the operation of heating it slowly and uniformly to the necessary degree to soften it, or to relieve internal strains, or to secure uniformity of texture.

Now we have to define steel.

The question, What is steel? was left thoroughly mixed by the discussions which took place in 1876 and 1877. The law, however, says now, that "steel is iron which has been produced by fusion by any process, and is malleable."

I offer a new definition—Iron is a liquid, congealed to a solid at ordinary temperatures.

This definition was first suggested by U. S. Senator John T. Morgan who enforced it with an able and exhaustive argument. It was suggested next, and independently, by Professor John W. Langley, of the University of Michigan, Ann Arbor.

Professor Langley was also the first to observe the varying rate of expansion, due to increase of temperature, between high steel and low steel. He also was first to note the presence of free oxygen dissolved in iron, a discovery which was received with ridicule in this country in 1877, and which has been confirmed by eminent European chemists within the last two years. This liquid, which we will consider now in the form known as steel, congeals at a high temperature; as it congeals it crystallizes in as many forms almost as are to be found in snow-flakes. The sizes and forms of the crystals are affected—from fusion—

First.—Largely by the rate of cooling; slow cooling favors the formation of large crystals, and quick cooling produces small crystals. Chernoff observed further that agitation produced fine crystals, and gave this as the reason why a heavy hammer, thoroughly and quickly applied at the right time, produced fine grain, increased density and greater strength.

Second.—The size and form of the crystals are affected by the foreign substances present.

We are not yet familiar enough with the effects of all of the components of steel, to be able to read off a complete analysis by looking at a fractured ingot; but the effects of carbon are so clear, so exact, and so

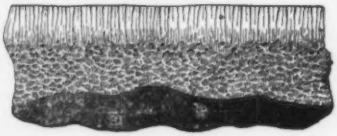
unvarying in well-melted ingots, that experience enables us to select our fifteen tempers with absolute certainty, and a skilled operator can split these fifteen into thirty tempers; so that it is a fact that when ingots are high-tempered enough to be fractured, they can be analyzed for carbon at a glance—much more accurately than it can be done by any known chemical system of color tests.

Third.—The crystals are affected by the walls of the mould in which the liquid is cooled.

This is very marked in chilled iron, and in what the melters call scalded ingots. The effect of the wall can be noticed also in any casting.

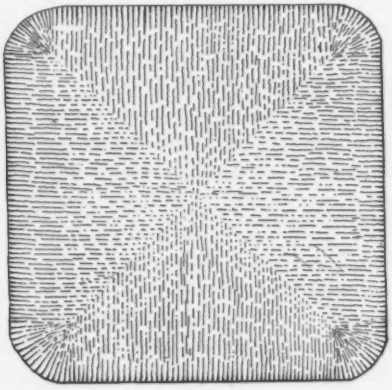
When steel congeals, the foreign substances are driven out to some extent by sudden cooling, just as cold ice is clearer and sounder and stronger than slowly-formed ice.

This can be seen plainly in chilled iron, where the graphitic particles are found just at the edge of the chill, thus:



The normal lines show the crystals of the chill; the heavy dots the particles of graphite that are driven off; and the fine dots the amorphous gray iron.

A scalded ingot looks like chilled iron, having this appearance:



But the crystals are not hard like the crystals of a true chill, and it would seem better to call such a body a polarized ingot.

A polarized ingot is very weak and brittle, so also is chilled iron very brittle.

There is much evidence to show the tendency to the extrusion of foreign elements as molten steel cools, of which the two cases following are given.

An ingot weighing several tons was drilled at the top and bottom, and analyses gave—

	For the top.	For the bottom.
Silicon	.023	.078
Phosphorus	.014	.032
Sulphur	.023	.027
Manganese	.306	.425
Carbon	.725	.775

A large bar of steel made for rolls by Krupp, of Essen, was turned and bored, and the turnings were analyzed, with the following result:

	Outer.	Inner.
Silicon	.130	.195
Phosphorous	.044	.050
Sulphur	.000	.005
Manganese	.448	.425
Copper	.234	.224
Carbon	.852	1.020

The latter case is not so marked, except in the carbon, as in the case of of the ingot, yet these two cases indicate that the elements sink as if by gravity, and leave the surface as it cools.

In the light of the liquid theory, the above cases illustrate a reason for the well-known unequal distribution of the elements in steel.

They also point to the idea that the elements in steel are there as alloys or in solution, and not in chemical combination. It may be true that there is a definite carbide of iron in steel, yet if there is, it is evidently there in solution.

So far as I have been able to observe the facts, during an almost daily experience of more than twenty-eight years, there is no property of steel that is not common to cast-iron; as, for instance, the hardening of steel and the chilling of cast-iron, and softening of either by heat; and I

believe that from the mildest steel, containing only traces of carbon, to the highest cast-iron, we have simply one substance, iron, containing various quantities of alloys or substances in solution, and that the properties which we observe vary only in degree, due to the quantity of alloy that is present.

Let us consider now the effect of temperature on steel.

It is well known to all workers in steel and cast-iron, that the whole structure of the ingot or casting varies very decidedly with the temperature at which the metal is poured, and this fact is constantly made use of to produce desired results.

But outside of those who have a large and varied experience with steel, it is not so generally known what a sensitive substance it is, both in volume and structure; and that in every piece of steel that is in existence to-day, there is a sure record of the last temperature to which it was subjected, as well as of the manner in which the steel was worked.

I mean to say that for every variation of heat which is visible to the naked eye, there is a corresponding variation in structure, which is equally visible to the naked eye if the record be opened by fracturing the piece.

Professor Langley's research on the specific gravity of differently heated pieces of steel (*American Chemist*, November, 1876,) shows that there is also a different specific gravity for each difference of structure.

This being the case, there is, of necessity, a permanent internal strain for every variation in specific gravity, because each change in specific gravity means, of course, a corresponding change in volume.

These strains vary from the slightest up to those that produce rupture; the piece cracks.

A piece of .53 carbon steel will vary in specific gravity from 7.844 to 7.818 from the bar finished at ordinary red heat to the bar cooled from a scintillating heat respectively, a difference of .026. A bar of 1.079 carbon under the same conditions will vary in specific gravity from 7.825 to 7.690, a difference of .135.

This shows that for a double quantity of carbon we have five times the difference in specific gravity, due to an equal difference in temperature. This is the "mystery" of the brittleness, and the tendency to crack, in high steel. If engineers who are in the habit of dealing with structural steels are disposed to think that these are both cases of high steel, I will explain that these particular experiments were made on

a fine grade of tool steel, and that compared to the ordinary Bessemer and open hearth steels, the .53 carbon tool steel would grade in softness and ductility about the same as .25 carbon Bessemer steel.

Experience teaches us too, that this rule of change in volume holds good all the way through the carbon series. A piece of .10 carbon steel may be heated white hot and plunged into water without breaking it; but if the same piece be quenched at a red heat, and also at a white heat, in different parts, and the parts are then broken, the different grains of the pieces will present a record of temperatures which once seen will never be forgotten.

On the other hand, if a piece of steel of 1.079 carbon be quenched at a bright orange color, it will be a very remarkable piece of steel if it does not fly to pieces. Granting these facts, does it follow that a piece of steel which has been unevenly heated, and so left in a strained condition, is injured irretrievably? No. This question brings us to the subject of annealing, a consideration of which will bring out some of the most useful and important properties of steel. Every piece of steel is at its best in all physical properties when it has been so annealed that it is in the condition which steel-makers call refined, that is to say, when the grain is in the finest condition possible, or when its crystals are the most minute and most uniform in size.

This statement is subject to a slight modification in considering a piece of hardened steel; when steel is hardened properly, the grain is slightly finer than it would have been if it had been allowed to cool slowly, but the difference is very slight, and if the hardened piece be subsequently annealed this difference disappears.

Each temper of steel refines at a different temperature.

A piece of .10 carbon steel will refine probably at a lemon color. I am not sure about this temper.

A piece of .30 carbon steel will refine at a dark lemon or bright orange color. A piece of 1.00 carbon steel will refine at a dark orange color, or the color that is reached just as the last shades of black disappear.

As a rule, the best heat to harden at is the refining heat, and the same heat is a good guide for annealing, although the heat may be raised very slightly in annealing high steel, but it should be done with great care, and it should be lowered considerably in annealing mild steel to avoid over-annealing. It is a remarkable, and probably the

most important property of steel, that no matter what the grain may be, no matter how coarse from over-heating, or how irregular from uneven heating, if it be heated uniformly to the refining heat and kept at that heat long enough, the crystals will change in size and will all become small and uniform, so that the fracture will be so even that it will be called fine-grained and amorphous. I do not like that word amorphous, however, because the magnifying glass will reveal a crystalline structure in the most beautifully refined steel. If a piece of chilled cast-iron be kept at a bright red heat for an hour or two, the chill will not only become soft, but the long crystals will disappear altogether, and the whole piece will be ordinary looking gray cast-iron.

If a scalded or polarized ingot be kept at a bright red heat for an hour or two, it also will lose every trace of its needle-like polarized crystals, and will become a uniform fine-grained piece of steel, and it will be as tough as if it had never been scalded and brittle.

If any ingot be annealed properly, it will lose every vestige of its distinctive carbon crystallization; it will become refined and tough.

Unannealed ingots are brittle, easily broken with a sledge, and are distinctively marked; annealed ingots are fine-grained and tough, and must be cut with a set to be broken; and when broken, an effort to grade them by the fractures is the wildest guess-work, in which none but a great expert should indulge. If a well-annealed piece of steel is the best piece of steel in every respect, an over-annealed piece of steel is the very worst piece, and should always go right back into the melting furnace. Over-annealed steel is brittle, harsh, not ductile, will not harden and will not temper, and I know of no way but melting to make it good.

A friend of mine told me he had started a wire-drawing shop. He was trying to draw his own dead soft open-hearth steel; he knew it was of excellent quality, but he could not draw it at all. It was not ductile, it broke, it did everything but go through the dies, and he asked me if I would help him out of his trouble. I took him to the wire-shop and first pointed out the annealing furnace; he said, "Do you anneal at that heat?" I said yes. "What is the temper of your steel?" I told him a hundred carbon. He replied, "I am going right home; I don't want to go any further; there must be some fools up our way."

Over-annealing is caused by too much heat too long-continued. I think time has more influence than temperature, but whatever the cause

may be, it changes the entire nature of the steel, and there seems to be no remedy.

The time required for annealing is arrived at by experience, and the time must be longer for a large piece than for a small one. I suppose the rule is, that the whole mass must be brought to the right heat, and must be kept at that heat but a short time; but I have no exact data to give, and it would be wrong to give any guesses that might be misleading. I cannot tell from analysis what change has taken place in over-annealed steel to make it so worthless, but I think it is probable that the injury is caused by the absorption of gases, because it is necessary sometimes to submit excessively hard steel to a longer heat than would be safe in an open fire, and this is accomplished safely by burying the steel in fine sand.

We can consider now the much discussed question as to whether steel and iron crystallize in service after a long duty, and having been subjected to many repetitions of strains, vibrations and shocks. If it be true that the largest crystals and the coarsest and weakest structure are formed when iron and steel are allowed to cool slowly and in a state of rest; and if the finest crystals and the best structure can only be formed by quick cooling and the violent agitation of the hammer or of the rolls; or by careful heating to just the right temperature to cause the formation of fine crystals, it would seem somewhat anomalous to assume that this is all reversed in the cold state, and that cold iron and steel can be shaken up into coarse crystals and a weak condition.

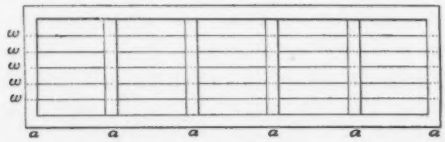
It may be possible that such an anomaly could exist, but it seems more reasonable to suppose that when an axle or a crank-pin breaks and develops in the interior large, fiery and weak crystals, that those crystals were formed there by too much heat, too slow cooling, and too little work when the piece was formed.

It is proper to remark here that the hammering of a round piece between flat dies is a dangerous operation; it is a common thing to find round hammered bars of steel burst in the middle for long distances, of which there is no evidence at the ends or on the surface; therefore, round pieces for structural purposes would be safer if they were hammered in swedges or rolled in grooves.

Piped ingots should be looked after too; it is quite likely that the hollow rail that broke with such disastrous results in New England lately was rolled from a piped billet.

It is but fair to add here that Professor Langley has observed that acids and the fumes of his laboratory will change the very structure of metals.

He had in his laboratory a frame of the following form:



The wires *w*, of copper, brass and German silver, were run through the cross-bars *a*, which were of wood, about 2 inches wide. After three years he noticed the wires breaking, and upon examination he found them to be coarsely crystalline, brittle—in fact rotten, and entirely changed in structure.

He found also that the parts that were in the wood, and so protected from the fumes, were soft, ductile, and entirely unaffected. All of the exposed wires were affected similarly, and all of the protected parts were equally unaffected, except the copper wire, which was stiffened, but not materially changed in structure.

We come next to the consideration of some of the physical properties of steel.

Mild steel, such as is used commonly for structural purposes, is more ductile, stronger, and tougher than iron; it is more easily and safely produced in large masses than iron, and when worked properly it can be put into the most difficult shapes and be made to do good duty.

High steel is hard and brittle, and generally of great tensile strength; its use is hazardous, because of its great change of volume for a slight change of temperature; yet it can be made very ductile by careful annealing, as is illustrated in the cold-rolled, cold-hammered, and cold-drawn samples before you, which contain 1.00 carbon, and have been worked cold into their present shapes.

This cold working has not injured the tempering properties of the steel, these being samples of ordinary clock spring, hair spring, and drill-wire steel. Hot working of steel increases its specific gravity, and cold working reduces its specific gravity; therefore cold bending of structural steel is not good practice, unless it be annealed afterwards.

When steel is to be subjected to repeated deflections or alternations

of strain, the mild steel is the more enduring, if we can accept the conclusions of Mr. Benjamin Baker in connection with the Forth Bridge work; but when steel is to be subjected to rapid vibrations, as in a pitman or a hammer rod, a higher steel is much better than dead soft steel. In using higher steel, however, it is important to have it well annealed and perfectly smooth.

I have known a sound and good hammer-rod to break in an unusual place from having a fine lathe tool mark left on it, because the scratch formed a check to the free vibrations and allowed a crack to start, which ruptured the rod. This rod was not annealed.

No sharp angles or corners should ever be allowed in structural steels, because there is no fiber in steel; it is crystalline. Admitting the truth of Mr. Baker's conclusions in regard to alternating and torsional strains as he applied them, we cannot apply them to the deflections and torsions of springs.

A great railroad company discovered not long ago that the moduli of elasticity of mild, medium and hard steels, tempered and untempered, were practically the same. Next it was decided that the strains in coiled springs were torsional; then the moduli of elasticity were applied to the formula for torsion, and it was discovered that if the bars were of the proper size it would make no difference how much or how little carbon they contained, nor whether they were tempered or untempered, the springs would be all right.

Finally it was specified that no spring should contain less than .90 carbon, and, of course, they were to be tempered. This may sound absurd, but it only proved the wisdom of the engineers; they were smart enough to test their own formula, and the result was a well designed set of springs and an admirable specification.

Mild steel does not afford good resistance to abrasion, it is too ductile and flows too readily; the flow causes heating and increased friction, and the low tensile strength yields to the friction.

Dr. Dudley's famous paper on rail specifications and the wear of rails, proves this when it is interpreted properly, and subsequent experiments in Europe give, I think, without exception, the result that the hardest rails show the least wear.

Probably the greatest test of resistance to abrasion and flow is found in the wire-drawing die. These dies require to be made of excessively hard steel, as is shown by the following analysis of two unusually good dies.

	Wire Dies.	
Silicon	.013	.119
Phosphorus	.014	.019
Sulphur	.009	Trace
Manganese	.396	.356
Copper	.....	.019
Carbon	1.975	1.92

It will be observed that these might be called cast-iron, were it not for the fact that they are good malleable cast-steel. Such a die drew the other day, in my presence, a coil of 1.00 carbon wire 1 160 feet long from .105 to .092 inch diameter, and the diameter did not vary in the whole length of the coil .00025 inch.

The effect of the chemical constitution of steel is very marked, and is well defined in high-tool steel, but it is not so well defined in mild steel, nor in Bessemer and open-hearth steel, therefore engineers do well not to meddle with chemistry at present; but it is safe to assume, in all cases, that the nearer the steel comes to being pure iron and carbon the better it is. The last remark has no application to what might be called alloy steel; there are possibilities in the alloys of iron and other metals far beyond our present knowledge, but it is not the intention in this paper to indulge in speculations.

It may be gathered from what has been said, that in general it is better and wiser for engineers to adhere as closely as possible to mild steel for large structures, where the material is used in comparatively large masses.

First, because it is more ductile than high steel, and therefore not so liable to break under sudden stress; and second, because it can be safely worked into shape by less skilled hands than are required in the manipulation of high steel; yet there are cases where it is wise to take advantage of the superior strength of high steel in the largest structures, of which we have notable examples in the staves of the arches of the St. Louis Bridge and in the wire of the cables of the East River Bridge.

On the other hand, there seems to be danger in the enthusiam of some of the admirers of mild steel, whose statements that it will stand any amount of "abuse and punishment," etc., may mislead them and others into the idea that it can be handled without even as much care as is ordinarily bestowed upon wrought-iron.

If the statements made in this paper are accepted as facts, it must be

obvious that care is always necessary, especially as regards heat, and particularly uneven heating.

I will give one case out of hundreds that have come under my notice to illustrate this point.

An engineer and bridge-builder brought to us a piece of steel about six inches wide and two inches thick, clean fractured. It was part of an eye-bar which had broken under a load which was, I believe, below the required elastic limit, and the engineer thought it could not be the mild steel it was supposed to be. The fracture was perfectly sound, and the grain was uniform and of proper luster, except on one corner, where in an area of less than one square inch were to be seen the large, bright, lustrous crystals, which registered "white heat."

We tried in various ways to harden the piece, but it was persistently "dead soft." Then we told our friend that that same thing would happen every time that his men heated up a corner of steel into the condition of that fiery corner.

Those weak crystals could have been annealed out of the bar, but it would have been a great deal better not to have put them there at all.

In reference to steam boilers, so far as strains are concerned, it would seem that high steel would be the best, but when we consider the daily alternations of heat and cold to which boilers are exposed—fire on one side, water on the other, mud deposits inside and bags outside—and the general ignorance of physics of the men who handle them, it is obvious that mild, tough steel is the only kind that is safe, and the milder and tougher the better.

Now as to guns.

The recent reading of the paper by Mr. Edward Bates Dorsey, M. Am. Soc. C. E., before the United States Naval Institute, has, for the first time, brought out from the advocates of built-up guns, clear statements of their views and the data upon which they are based.

All who read the admirable and temperate remarks of Lieutenant R. R. Ingersoll, Captain Rogers Birnie, Jr., and Lieutenant Austin M. Knight, must be convinced that these gentlemen have studied their subject thoroughly in regard to the properties necessary to a good gun, and with equal thoroughness, from a European standpoint, they have convinced themselves as to the best way to make the gun.

Messrs. Morgan and Davenport have shown also their exceeding skill in making the material which these gentlemen require.

Captain O. E. Michaelis, M. Am. Soc. C. E., Corps of Ordnance United States Army, however, seems to be inspired by the spirit and courage of Rodman, and knowing that Rodman started out with the right principle, he proposes to take up that principle where Rodman left it and carry it on into the higher domain of steel, where great success and great honor await the man who makes the first gun of the future.

My preceptors, Wade and Rodman, held that the qualities required in a gun were elasticity, springiness and power to resist abrasion, combined with high strength and a power to offer a uniform resistance in every direction to all of the strains to which it might be subjected.

All of the properties were reached in the highest degree possible in the material with which they had to work, and none of their guns ever failed.

If Rodman had lived, the advent of good steel in great masses would at once have been seized upon by him, and before now he would undoubtedly have cast the best and biggest, the safest and the cheapest guns that were ever made.

Rodman was a true engineer, and it was a cardinal principle with him, that any gun had a certain number of foot tons of work to do, whether it were to batter down an earthwork or to sink a ship; and he always claimed that the best gun was the gun that would do this work for the fewest cents per foot ton, including in the cost the making of the gun.

Some more modern writers seem to scorn this question of cost, and at the same time to insinuate that civilians have not a dollar at stake in the gun question anyhow.

Possibly not; yet there may be civilians who have a stupid notion that they can trace the ultimate cost down into the taxpayer's pocket, and the taxpayer may be thick-headed enough to think that he is helping to pay for his own protection.

When an officer berates those misguided people who delay appropriations, by making suggestions as to the best way to use the money, he reminds one of the average minister of the gospel, who when he is criticised, always says: "Now you stop, it is wrong for you to do that, you are interfering with my spiritual work."

I have not a word to say against the views given by the officers who discussed Mr. Dorsey's paper, as to the proper characteristics required

in a metal which has to endure the strains to which a gun is subjected. That part of the subject they understand thoroughly.

I cannot imagine how a good gun could be made of dead soft steel. The bore of such a gun would enlarge from the first round; the lands of the rifling would give way under the pressure of the projectile; the vent would wear out rapidly; and altogether I should expect that after a hundred rounds such a gun would be about as symmetrical as an old battered hat.

My objection to proposed methods is: to the building-up system; the notion that "definite shrinkage" is a practical possibility; the idea that rings can be so shrunk together that each shall be strained to exactly its elastic limit, when in fact that elastic limit cannot be known; the enormous cost of unnecessary operations, and the doubtful utility of the operations after they are performed.

Lieutenant Ingersoll says:

"What we want with gun-steel is uniformity; but this should be a development with high rather than with low qualities, and the tendency and march of events indicate that this will be attained by: First, a more intimate chemical knowledge of steel; Second, a less barbarous forging-machine than the hammer; Third, annealing; Fourth, oil-tempering."

As to the "First," when the departments begin to dabble in the chemistry of steel there will be no more guns made; what is needed instead is an intimate knowledge of the physics of steel.

To the "Second," all will agree who know anything of the subject; and we may add, we want a less barbarous forging-machine than the hydraulic press. We want no forging-machine at all, the steel can be made to forge itself by static pressure and by heat.

To the "Third," there can be no objection, as there is no known way of getting improper strains out of high steel except by annealing.

The "Fourth" is of doubtful utility. It is not probable that the benefit derived, if there be any, can compensate for the cost, especially when we reflect that the parts are annealed subsequently. What is the object of the annealing? Mr. Davenport answers that as follows:

"It is generally admitted that the effect of tempering in oil or any other liquid is to *fix*, by rapid cooling, the amorphous conditions existing in the heated mass, thus preventing the formation of a coarsely crystalline structure, and destroying the irregular and more or less crystalline condition existing in every forging of considerable size when it leaves the hammer.

"Besides this, the molecular condition of the mass is far more uniform after treatment than before."

Thus is the whole forging business effectually damned by the trusted defender of the system, whose unquestioned skill and success in this hazardous business entitle his statements to the fullest credence. Lieutenant Knight quotes the lamented A. L. Holley, M. Am. Soc. C. E.; let us quote him a little farther than the gentleman went. Holley first quotes Chernoff's experiment: "A coarse-grained, sound, cast-steel ingot was cut lengthwise into four parts. One of the quarters was cut, in a lathe, into a test bar; the second was heated to a bright red, forged under a steam-hammer, the forging being stopped while the piece was yet rather hot (probably cherry-red); the third piece was heated up to the point at which the hammering of the preceding piece had been left off, and was allowed to cool slowly. The fracture showed a very fine grain similar to that of the forged piece. These two quarters were also turned into test-bars."

The results of the tensile tests were as follows:

	Breaking load.	Elongation.	Dynamic resistance.
Unforged	34.8	0.023	0.8
Forged	41.5	0.053	1.1
Annealed	38.7	0.166	3.21

Dynamic resistance in tons. Ultimate strength + ½ elongation.

"The obvious conclusion is that it is possible to make a steel in its cast state just as strong as if it had been hammered."

Next he gives a "bored, annealed, and tempered-in-oil" "gun-tube."

	Limit of elasticity, Tons per square inch.	Tension at rupture, Tons per square inch.	Elongation, per cent.
At the back, No. 1	22.0	42.5	11.1
" " 2	22.2	39.6	8.7
In front, " 1	22.5	38.1	15.1
" " 2	22.7	38.5	15.0

The New Navy Specifications Require for Tubes—

	Limit of elasticity, pounds per square inch.	Tensile strength, pounds per square inch.	Elongation, per cent.	Contraction of Area.
A.....	38 000	80 000	22	35
B.....	34 000	72 000	20	20
C.....	33 000	70 000	12	15

The unforged pieces are very close to the requirements. Holley gives other data, and then says of a test of the above tube, which was fired 100 rounds of heavy charges:

"No flaw of any kind was discovered, and the deformation of the chamber was found to be less than half the average in forged-steel tubes."

He then continues:

"It should appear * * * that the American system of cheap ordnance—cheap because it is cast—is to be successfully realized.

"If so, it will follow that the just criticism upon the standard American gun, that it is comparatively worthless because it is cast-iron will be reversed.

"We can hardly conceive of a fact of greater magnitude, from a defensive point of view, than this: that while the United States has at this moment not a single standard type of naval gun, or gun of position, that is comparable in efficiency with the guns of foreign states, it has, by means of the good policy of its Ordnance Department, studied the results of foreign experiments and avoided the enormous cost of original investigations, and that this policy must be now rewarded by the establishment of the cheap cast gun, the metal to be, not crude iron, but steel having three or four times the strength."

He then gives the cost of guns in 1865:

Armstrong 10.5 inch wrought-iron hoop gun....	33.6	cents	per pound.
Krupp 15-inch solid steel gun.....	87.5	"	"
Blakely 10-inch steel tube, hooped with steel....	78.5	"	"
Whitworth 7-inch steel tube, hooped with steel..	64.5	"	"
Parrott 10-inch cast-iron, wrought-iron hooped..	17.0	"	"
Rodman 10-inch cast-iron.....	9.75	"	"
" 15-inch "	13.2	"	"

Holley continues:

"The hammering of a large mass of steel—for instance, a forty-ton ingot for a gun or marine shaft—is a very costly and hazardous undertaking. * * *

"Forging, under the heaviest hammers, reaches only the parts in the immediate vicinity of impact; the piece is therefore subjected to a series of internal strains due to the difference in the molecular arrangement of adjacent parts.

"It is thus left subject to internal strains which may cause ruptures when and where least expected."

We might be told just here that the Terre Noire people have not succeeded in producing good cast guns.

We know it. But they never carried out Rodman's principles, and therefore they have never tried what we advocate.

For the information of any who may not know, I will say that the Rodman plan consists in casting a gun on end, breech down, with a hollow core. Water is circulated through the core to cool the interior rapidly, and a strong fire is kept in the pit to keep the exterior of the gun warm, thus forcing the metal to contract all in one direction and on the interior. The operation is so simple, so easy, so sure and so scientific, that it is beyond criticism, and it would seem superfluous to add any further arguments than those given in the early pages of this paper, to make clear the possibilities of this process, properly applied, to steel. There are plants in the country now which only require the addition of some pits and moulds to prepare for the casting of 40-ton guns for trial; and the extension of these plants to the casting of 100-ton or 150-ton guns would cost but a comparative trifle.

The cost of one huge hammer, or one hydraulic plant, would build a half dozen casting plants.

Splendid steel castings up to thirty and forty tons weight can be bought now for less than six cents a pound, while we are told to think of forty cents a pound as the price of rough-bored, rough-turned, annealed, oil-tempered and annealed gun parts. My own opinion is that forty cents a pound is not a high a price for such work.

To this price must be added probably forty cents a pound more for the cost of finishing these much treated parts; this brings us very close to the figure given for the Krupp gun.

Now the people of the country are asked to bid on 1 310 tons of gun

parts, and they are invited to build enormous plants to make them, and they are to be allowed thirty months in which to prepare.

1 310 tons at $800 per ton = $1 048 000.

This is a large sum of money, and if the work can be done for half of it, and the other half could be made to build the plant, the contractor would come out with his plant for profit, minus the interest and wear and tear.

Not a bad state of affairs if the plant were worth anything; but as such a plant could have no commercial value, since there would be no commercial use for it, as soon as Congress tired of making appropriations or the departments changed their minds about the style of guns they wanted, the plant would be a scrap heap and the profits would vanish into nothingness.

In conclusion of the gun subject I repeat what I have said before: I believe the built-up gun to be unscientific and unmechanical. To settle the question of the mode of manufacture, before the Government goes to enormous expense to build such guns, and before private parties make the hazardous investments involved in building the necesary plants, is it unwise to suggest the expediency of spending two or three hundred thousand dollars in testing the Rodman plan applied to steel, not merely because it is cheap, but because it is full of promise, and it is pre-eminently American? It served us well in our hour of extremity, and it laid the world at our feet.

It has cost Europe probably a hundred millions of dollars to produce an uncertain gun of greater power, and we may hope that it will cost her another hundred millions to produce something to equal America's gun of the future.

For ready reference, for the use of engineers, the statements made may be summarized as follows:

Iron and all metals are liquids.

Cold steel is congealed iron, containing in solution various ingredients, which give to it certain marked properties.

Heat is the power which gives to steel all of its good and all of its bad conditions.

Steel changes in volume and structure with every degree of heat that is applied to it, and the changes may be read in the fracture as surely as we read the changes in volume in the mercury column.

Slow, quiet cooling from a high temperature causes the formation of large, irregular crystals, and renders the steel weak.

Quick cooling and agitation, form small, uniform crystals and a strong condition.

The application of heat alone will change the form and the size of the crystals.

The change of volume due to a unit of heat increases as the content of carbon increases; therefore high carbon steel must be handled with exceeding care.

The temperature to which it was last subjected, moderated by its subsequent treatment, is always recorded in the structure of steel, and may be read there if the piece be fractured.

Annealing, making soft, ductile and uniform in texture, is the most important of all operations from an engineer's point of view.

Although annealing will relieve strains and change a coarse structure to a fine uniform grain, it must not be supposed for a moment that any amount of annealing will heal a rupture.

I do not believe there is any cure for a rupture, because I do not believe steel can be welded.

Steel being crystalline, has no fiber; therefore there should be no sharp angles, no sharp edges, and no unfilleted corners; the surfaces should be smooth and free from tool marks or indentations caused by sledge blows and the like.

With our present knowledge, the BEST STEEL for structural purposes is that which is most nearly composed of iron and carbon.

Finally, good steel, properly worked, is the most useful of all of man's productions, and it may always be relied upon to do its full work to its utmost limit; but if the laws of its being be violated, it will as certainly respond, causing disappointment and disaster.

DISCUSSION.

Lieutenant J. W. DANENHOWER, U. S. N.—I had not expected to speak this evening, and if I had I could not have come prepared to speak upon the chemical and physical properties of steel, which have been so ably treated by the writer of the paper under discussion. As to guns, I have had, certainly, experience. Mr. Metcalf has spoken ably about a theoretical gun, and I venture to say for myself, and for my brother officers at Annapolis, that we would be very glad indeed to have such a gun, particularly if it were cheap and convenient, and made of cast-steel on the Rodman principle, if that is possible.

There is one question that I would like to ask Mr. Metcalf, and that is: What becomes of the compression of the Rodman gun after the gun is annealed?

Certainly we are very willing to look to engineers of experience in building structures to aid us in this direction, and to look to steel manufacturers of the country to provide the material, and, if practicable, cast it as has been suggested.

You will remember that after the war we had a vast stock of ordnance on hand. Then followed with us about ten years of inaction, although foreign governments were developing armaments and arms. Then, when it was demanded by economy, we took the 11-inch gun, bored it out to thirteen inches, and inserted a cast-iron tube; put a ring on the muzzle to keep it in place, and used that gun very successfully. Our muzzle-loading rifle did better work than the corresponding gun in the English service. Initial velocity was a requirement of that caliber of gun ten years ago. An initial velocity of 1 400 feet per second was thought sufficient then, and, indeed, was deemed high; now we require an initial velocity of 2 100 feet.

It was found in Washington, after 1876, that the hooped guns did useful work. Krupp has made some 17 000 guns, and only 18 have burst.

The navy is limited in the length of its guns, because guns of too great length are not serviceable on shipboard. The 6 and 8-inch guns have met with success. We have one which has been fired 274 times without showing any sign of weakness. Those at Annapolis have been fired as rapidly as possible with a force of only ten men, and those only ignorant, ordinary hands; this shows how readily they can be managed. The 6-inch gun was fired ten times in eleven minutes. The initial velocity was 2 105 feet, while the pressure was only 14½ tons at the breech-chamber. That is what we desire, a good gun with a high initial velocity. We think we have got it now, and we want a good trial of it.

Two or three years ago Congress made an appropriation of $24 000 for casting three guns, one of open-hearth, one of Bessemer, and one of crucible steel. That is not a very large appropriation, but it is a pretty large slice out of the army appropriations to run the Bureau in Washington for ordnance for the navy.

There is little more that I can say, except this, that I am very glad this meeting has taken place, and that such men as those that compose the American Society of Civil Engineers have taken an interest in this subject.

Mr. WILLIAM METCALF.—As to Lieutenant Danenhower's question as to what becomes of the compression of the Rodman gun after the gun is annealed: Well, what becomes of the hammering after the gun is annealed? In making a gun, if the first preparation by annealing does not give that condition of strength and fineness necessary, it is perfectly obvious to any one that a simple way to get it is to heat it to the proper point, and bring every part of the steel to the best condition, then cool from the interior of the gun. It is a great deal cheaper and easier to apply heat than a hammer. It seems so simple that I almost wonder the question was asked.

As to being a theorist, it has amused me a great deal to be classed among theorists. I have made in the past years a hundred thousand tons of guns.

To justify the position I have taken, I want to say that my remarks apply to heavy guns; that is what I have talked about. The 6 and 8-inch guns are not heavy guns, they are little things. The difficulties increase as the square of the size, as Mr. Danenhower very justly remarks, in regard to the strain. It is a fact in regard to the hundred-ton gun, that the tensile strength they afford is of an extremely low grade.

Before the discussion goes any further, I would like to say that if we call the gun a steel cast gun when talking of it, instead of a "cast-steel" gun, we would be clearer in our argument. There is an abundance of cast-steel to-day, but no steel cast guns.

I said distinctly, and if I tried to make anything in the paper clear, it was as to the effect of hammering large masses; all of that hammering is worthless. All of the best properties of steel can be obtained by annealing from the ingot without any forging. I have showed that heat affects steel as certainly as it does a column of mercury, but after steel has been heated no one can know the elastic limit without testing for it; to assume that it is a certain amount, because some supposed to be known temperature has been applied to it, is mere guess-work.

Commander H. B. ROBESON, U. S. N.—I had not expected to be called upon this evening, and came as a listener without intending to take part in the discussion.

I have been very much interested in Mr. Metcalf's paper, and have

learned a great deal in regard to the working of steel, and its application and use in the manufacture of ordnance.

There is one point however which has not been touched upon this evening, and that is in regard to the weight of the proposed steel cast guns. This is an important consideration, especially in the construction of guns which are to be mounted in our modern ships.

In the cruiser or ironclad of the present day, the increased weight of coal, engines, armor, and the mechanical appliances necessary to promote the efficiency of our floating fighting machines, renders it imperative to utilize to the utmost every ton of displacement; and for this reason, if for no other, it is of great importance that no unnecessary weight should be added to the battery.

The increased length given to modern ordnance, in order to obtain the required muzzle velocity without unduly increasing the pressure in the bore, also renders necessary an increase of weight in the different calibers; and the built-up system was employed so that the gun could be made of steel having the requisite elasticity and a tensile strength, giving maximum durability and safety with a minimum weight of metal.

It undoubtedly will be a great advance in gun construction if the Rodman system can be successfully applied to the fabrication of steel cast guns. It would seem proper to say, however, that if this plan of construction is to increase the weight of the gun, it would be better adapted to ordnance mounted in forts and land defenses rather than on board ship.

I would like to ask Mr. Metcalf what would be the difference in the weight of the 6-inch guns, and whether he would propose to make a cast gun of the same weight as one of the built-up guns constructed by the Naval Bureau of Ordnance and now on board the Atlanta.

Lieutenant Danenhower has so thoroughly exhausted the subject that I can add but little more, except to say that it is a matter of great satisfaction to the officers of the navy that this question of gun construction has been taken up for discussion and is exciting so much attention.

Mr. WILLIAM METCALF.—As to the relative weights of built-up and cast steel guns, I have made no figures whatever. I do not know the dimensions of the 6 and 8-inch guns, having never seen either the guns nor the drawings of them. In all I have said about guns I have had in mind only large guns, say from one hundred tons upward.

Mr. R. W. HUNT, M. Am. Soc. C. E.—It is with great trepidation that I venture to say anything on the subject, particularly in this presence. I have not had any experience in making guns; but I may say from what I know of steel castings for other purposes, that I have a very strong hope that the theory of the steel cast gun will prove to be right. I think there is hardly an engineer in the country to-day who would trust a hammered steel shaft for large work. Experience has proven time after time

that they are unreliable, no matter how much they have been annealed after forging. High cast-steel is giving satisfaction for many purposes. I only hope that Congress, or the Navy Department, or the War Department, being accelerated by Congress giving it some money, will have some cast guns made from it.

An ordnance officer has said to me: "If you steel men believe so strongly in steel cast guns, why don't some of you get together and make some?" We are patriotic, but at the same time we are committed to the principle that the Government should pay for public works; the general public should bear the expense.

Suppose that during the several weeks this matter has been discussed, the Government officials should have said to Mr. Wellman, for instance: "You have an order from this Government to make a gun for us as big as your establishment will permit." He would have executed it directly, while we have been talking about it, and the matter would have been well towards settlement. Unfortunately that has not been the case and if it had we would not have had this delightful evening, nor the opportunity of meeting here in discussion.

I will not detain you longer; I can only say that I sincerely hope that somebody will give us a chance to try this steel cast gun, and that I consider Mr. Metcalf's paper one of the most valuable yet written on the treatment of steel. It is worthy of a place as a text-book in metallurgy.

Mr. GEORGE BREED, U. S. N.—Mr. Hunt remarks that no hammered steel in America has yet been found satisfactory.

Mr. HUNT.—I said I thought that engineers were not willing to place their trust in hammered steel shafts because so many of them have been found unsatisfactory, and that the tendency is to rely on steel cast shafts.

Captain O. E. MICHAELIS, M. Am. Soc. C. E.—The announcement of the reading of this paper induced me to make a long journey to-day for the purpose of hearing it, and the valuable information imparted by the author has fully repaid me for any trouble taken. To speak with authority upon the construction of steel guns in all its phases demands the conjunction of three conditions: the utterances must be founded upon the science of the engineer, upon the practice of the steel manufacturer, and upon the experience of the gun-maker. Among the few men who in their persons unite these three necessary qualities, the essayist of the evening occupies a prominent place. Three years ago I read before the Society a paper upon "The Heavy Gun Question," * in which I advocated the trial of a steel gun cast on the Rodman principle, and I can say little to-night that would not be a repetition of what I then stated. It has been repeatedly argued that if the steel manufacturers have such faith in the practicability of a cast gun, why do they

*Transactions, Vol. XIII, p. 215, July, 1884.

not fabricate one and present it for trial? Many rejoinders can be and have been made. Possibly the simplest way of answering the question is to ask another. Why do not those steel-makers who have faith in the built-up gun fabricate and present for trial, say a 12-inch rifle? They have a great advantage at the start. They know that the system is approved by those in authority; they have the benefit of the researches and experiments, ably and thoroughly made by bright officers of the navy and army; and they have at their command the advice and the sympathy of these officers. Why then under such favoring circumstances do these gun-builders not produce at their expense a heavy rifle? All the preliminary experience is at hand, obtained by hard study and at great expense. Why then do they not undertake the construction at private expense of great guns? The reply usually is that in the direction of built-up guns we are working on a certainty, and that therefore Government is justified in "footing the bill." This certainty, according to the bulk of published opinion, is founded upon the results of European experience; though in justice to a few officers I must state that they claim that their views are founded upon American experience, and that their methods are radical departures from European models. This question of European successful experience with steel built-up guns, upon investigation, is found to depend upon the great success of the Krupp guns. These guns are made of crucible steel, and the substitution of open-hearth steel, if based upon experience alone, is almost as decided a departure from precedent as the proposed hollow steel cast gun founded upon the success of the Rodman cast-iron guns. No comfort can be drawn from the experience of England and France with their built-up guns. As regards England, I need only name the Colossus, the Active, the Collingwood, and the Ajax. In France I am informed that two 42-centimeter DeBange guns failed in the proof at Ruelle. And that the question is not so absolutely settled as the opponents of a Government trial of a steel cast gun would have us believe, is shown by our own experience. Notwithstanding the fine and original work done by our ordnance officers; notwithstanding the ability and the enthusiasm exhibited by our steel-makers, the tube of the 8-inch army gun of English make, the second one sent, the first having been rejected, has proved to have been subjected to injurious internal strains, and no one can foretell the moment of its failure. The science, the ability, the experience that evolved the most powerful cast-iron guns, that to-day has done such advanced work with the shrinkage system, is still at our service, and an opportunity can soon determine whether or no a steel cast gun is a feasible construction. I contend that enough has been said and written to justify the recommendation to Congress of a liberal appropriation for the exhaustive trial of the plan.

Even in England they are beginning to believe "that with good metal a considerable approach might be made by judicious treatment towards the qualities imparted by forging."

Even if it be proved that serviceable guns cannot be cast, the cost of the experiment will not be a waste of public money, for the experience necessarily gained in casting large masses of steel will be for the general good.

Colonel Wm. C. Church—Mr. President: I labor under a very serious disadvantage. I was detained to-night and was not able to listen to the paper. I am sufficiently well acquainted with Mr. Metcalf's views, however, to be quite sure that I indorse what he has said upon this subject.

I think to produce the best results, our ordnance officers should revise what they have learned from a too exclusive study of European examples, and with open minds come into consultation with American manufacturers who unite a practical experience in the handling of steel with a theoretical knowledge of its properties; and especially with one like Mr. Metcalf, who has had in addition a large experience in the manufacture of guns. Let them not overlook the significance of the fact that it is the foreign manufacturers—Krupp, Whitworth, Armstrong and others—who have led in the foreign development of ordnance, and afford our American steel men a like opportunity to test their abilities in the direction of further improvement.

Lieut. W. H. Jaques, U. S. N.—It is always a pleasure to listen to what the author of this paper under discussion has to say, particularly upon the subject of the "use of steel in heavy guns." It matters not whether they agree or disagree with him, you always learn something. Therefore my visits to Pittsburgh are looked forward to with much interest, even if they do not compass a meeting at the Duquesne Club, whose seducing influences, as many of you know, leave only the most delightful memories.

There are many elements of satisfaction to be found in the paper before us, for in the author's statements to the House Commission on Ordnance and War Ships, November 23d, 1885, and to the Senate Committee on Ordnance and War Ships, December 10th, 1884, he intimated that he had withdrawn from any interest or further action in connection with guns or the subjects relating to them. To the former commission he said:

"I am not interested in any open-hearth plant and I am done with the gun business." To the latter committee he stated: "I have no interest whatever in either heavy armor or guns except as an American citizen. I never want to see another gun, so I write freely, wishing to give you all the information that is possible. The concern with which I am connected is entirely out of the line of such work, and, for myself, I have had enough of it, and like to think of the gun business as belonging to the dead past."

That he has again taken up the subject is therefore a matter of deep interest, particularly since we find his views modified to so great an ex-

tent as to accept the results obtained by Messrs. Morgan and Davenport, the good work of my fellow ordnance officers, and the "views given by the officers who discussed Mr. Dorsey's paper, as to the proper characteristics required in a metal which has to endure the strains to which a gun is subjected. That part of the subject," he says, "they understand thoroughly."

The author's treatment of the subject also from a theological or spiritual standpoint is a unique departure, and one which I will not attempt to follow.

Passing over the author's very interesting history of and opinions concerning the properties of steel and its use in structures, I desire to call attention to some of his statements made in this paper, and previously, which bear directly upon the supply of heavy ordnance. On page 42 of the Report of the House Commission already referred to, will be found the following, appearing as a part of his testimony:

"Inasmuch as the large steel manufacturers of this country are willing to undertake the manufacture of heavy cast steel guns, provided the Government furnishes sufficient money for the experiment, now in order to do away with all jealousy and prevent talk about rings and favoritism, the following plan is proposed: Let the Government offer three prizes, large enough to enlist the confidence of manufacturers, to be given to those who succeed in making the best 6-inch cast steel guns, this size being within the limits of present capacity of nearly all our steel plants. Three prizes also for 12-inch guns, to be given to the successful competitors for the first prize. This plan would save millions of dollars to the Government and give the best attainable results.

"Let the guns be made according to the method deemed best by the manufacturers. All the guns to be submitted to the same destructive tests, and classed according to endurance, as 1, 2, 3, etc. There should be no effort made to keep our mechanics within the circle of the experiments of the English, German, or French, but leave them free to act as they think best; and in five years' time the results obtained will show a progress in the manufacture of heavy ordnance that would astonish the world."

There are serious objections to this plan, as it would restrict competition to cast guns; and, as Hon. Thomas B. Reed, of Maine, remarked in the consideration of the conference report on the Fortifications Bill, March 3d last, "the iron gun foundry which required no additional expenditure for plant could produce a gun to compete, while the steel manufactories, which would require an expenditure of $2 000 000 for additional plant could not afford to enter into the competition." The cast-steel gun would have greater ballistic power than the cast-iron gun, but the competition would be limited to guns of the former character; and as far as a comprehensive test, or a comparison of cast and built-up guns is concerned, we would be no nearer a solution of the problem than before, and the jealousies would be greater than ever.

If the Government is to offer prizes for the most efficient ordnance, the field should be unrestricted, except by conditions of efficiency. It

would not be a difficult task for artillerists to lay down rules which would insure a thorough comparison of weight, economy, energy, penetration, etc. But let this competition be open to the world; to any kind of material; to any system of construction; and to any method of manufacture; the guns to be made at the competitors' expense, the Government paying a handsome reward to the successful one only.

The proposal of the Bethlehem Iron Company, March 22d last, describing the inauguration of a plan for the manufacture of modern steel gun forgings and solid steel armor, so long in advance of any positive inducement or guarantee from the Government that even a single contract will be awarded them, is a powerful and gratifying indorsement of the decision reached so long ago by the Naval Bureau of Ordnance that forged steel built-up guns were the best, and solid steel armor the most efficient.

This action of the Bethlehem Company in making such enormous expenditures is a practical lesson to the advocates of cast-steel ordnance, none of whom appear to have been willing to indorse their opinions by any financial risks.

Americans, as a rule, are not slow to undertake work if it contains a reasonable assurance of financial success. I therefore cannot understand why the Pittsburgh manufacturers, who have urged so firmly the use of cast guns, have not made them at their own expense and risk, if such guns can be manufactured so easily and so wondrously cheap as is claimed, and if such "great success and honor await the man who" will make them.

I heartily indorse the author's reference to the importance of the "question of cost," provided he does not sacrifice ballistic power. We must continue our estimates further than the first cost of the gun. A small increase, for instance, in the weight of a gun will largely augment the cost of application and use. From the beginning of my studies I have kept this carefully in view, and am confident that no recommendation of mine has omitted it.

The Naval Appropriation Bill, approved March 3d, 1887, contains a clause providing that

"Twenty-four thousand dollars, or so much thereof as may be necessary, may be used, in the discretion of the Secretary of the Navy, for the purchase and completion of three steel cast, rough-bored and turned, six-inch high power rifle cannon of domestic manufacture, one of which shall be of Bessemer steel, one of open-hearth steel, and one of crucible steel; provided that the castings for said cannon shall not be paid for until the cannon shall have been completed and have successfully stood the statutory test."

This will be a partial answer at least to the author's suggestion of the expediency of spending $200 000 or $300 000 in testing the Rodman plan applied to steel. The act is not very clear, but it seems to me that the entire responsibility of design, finish and test should be put upon

the manufacturer, and that the Department should not be required to accept the responsibility of design, finish and test while the manufacturer is only to make the rough casting. I beg no one will accept this reference as an under-valuation of the skill and experience required to make good castings, for I appreciate the difficulties that stand in the way of obtaining them. Terre Noire's experience alone will prove a weighty example.

The methods practiced in Mr. Metcalf's works in Pittsburgh suggests to me the following question. He says, "we want no forging machine at all; the steel can be made to forge itself by static pressure and by heat." Why then does he not send his products into the market without the enormous amount of forging he puts upon them by his rolls and hammers? Why does he not employ "heat," and what he calls "static pressure," instead of these forging instruments which he indicts as "barbarous?"

It will be seen therefore that the views of the author and those of the opponents of the theory of the equal or superior efficiency of cast-iron or cast-steel guns over built-up forged steel guns are not so wide apart, for Mr. Metcalf advocates that the best gun is the one that will do the greatest amount of work for the least money; that the qualities required in a gun are "elasticity, springiness and power to resist abrasion, combined with high strength and a power to offer a uniform resistance in every direction to all of the strains to which it might be subjected;" that "care is always necessary, especially as regards heat, and particularly uneven heating;" and concludes by saying that "good steel, properly worked, is the most useful of all man's productions, and it may always be relied upon to do its full work to its utmost limit; but if the laws of its being be violated, it will as certainly respond, causing disappointment and disaster;" all of which principles and opinions have been advanced or practiced by the authorities I named on page 81 of the discussion of Mr. Dorsey's paper.*

Whitworth, and Krupp, and Schneider have succeeded because they have heeded the laws that a long experience has provided; because they properly work and treat their steel; because they understand the benefits of annealing and the controlling power of heat; and they build up guns because they have acquired methods of performance which are both scientific and mechanical.

I thank you for your attention and for your courteous invitation to take part in this discussion.

Mr. WILLIAM METCALF.—In answer to Mr. Jacques' funny question as to why we use hammers and rolls in our business, I would say that our stock account shows that we have to make about 6000 different sizes and shapes of bars to meet the wants of our patrons, and we don't

* Proceedings United States Naval Institute.

know how to cast these shapes or to get them economically in any other way than the one we use.

I must say here that I cannot understand how the three-prize proposition became attached to my name in the Ordnance report. I never made such a proposition; it belongs to some other person.

Dr. R. J. GATLING.—The paper of Mr. Metcalf on steel, etc., certainly contains much valuable information. My opinion is, steel for gun construction should be neither too hard nor too soft, and should have the combined qualities of toughness, strength, and elasticity. Such steel should be made from the most carefully selected material, and should not be over-heated; indeed, the greatest possible care should be exercised in all the processes of its manufacture. Gun steel, in my judgment, should not be hammered, for the reason that when large ingots are placed under the hammer, the blows of the hammer will harden and condense the parts of the steel struck, and in a measure render the mass of metal less uniform in texture; and it may be doubtful if the best system of tempering and annealing will bring it back to a homogeneous state. Hard steel, which contains a large percentage of carbon, if used in gun construction should be heated and annealed with the greatest possible care; and it often happens, after undergoing the most careful treatment, it will be left under internal strain and will be liable to crack or break when subjected to sudden shocks or violent and repeated strains. Such being the case, I think a mild and tough steel of good quality, that is uniform in texture and possessing a high limit of elasticity, will be the best material for gun construction. At all events guns made of such mild steel will not be so liable to burst, and consequently will be safer to the men using them than guns made of harder and higher grades of steel. It should be borne in mind that the best qualities of steel of any kind can be made worthless as gun material by over-heating and over-annealing. I concur in the views expressed by Mr. Metcalf, that heavy guns of great power (especially for fort use) can be cast of a good grade of steel on the Rodman principle, that will have great "elasticity, springiness, and power to resist abrasion, combined with high strength and power to offer a uniform resistance in every direction to all the strains to which they might be subjected." Such guns would be able to do good service ashore or afloat, and would be a great deal cheaper than built-up guns made from a high grade of hard steel. Steel cast guns designed for fort use could be, if necessary, made of greater weight than guns designed for naval service. If guns for naval use can be made on the built-up plan, that have more power in proportion to their weight than steel cast guns, then and in that case it would be well for such guns to be made, regardless of their cost; but in these days of economy it would seem to be but the part of wisdom to have cast guns made of good qualities of mild steel for fort use, cast on the Rodman principle, which would be equal in power and

effectiveness to any guns in the world. Mr. Metcalf truly says: "If Rodman had lived, the advent of good steel in great masses would at once have been seized upon by him, and before now he would undoubtedly have cast the best and biggest, the safest and cheapest guns that were ever made."

Mr. F. COLLINGWOOD, M. Am. Soc. C. E.—The use of steel in large masses has been brought so prominently before the public within the last decade, that every engineer has been obliged to devote a certain time to the study of its qualities and behavior. Its introduction has been tentative, and its advocates have been forced to overcome the conservatism and the traditions of centuries.

The argument that has been most tenaciously held against its use, and which is not yet entirely overcome, is that of lack of uniformity. I remember very well a remark made to me by the late A. L. Holley, M. Am. Soc. C. E., who was an undoubted master in everything relating to steel, that there had "never been so large an amount of steel of the high grade and uniform quality required for the wire of the East River Bridge made under one contract;" but he ventured to say that "if asked for it would be produced." This was but about fifteen years ago.

The present controversy turns upon somewhat the same lines of thought. European engineers have become convinced that they can be more certain of the condition of the steel after its manufacture as to uniformity of composition and of strain, if it be cast in moderate-sized masses and built up into guns of the requisite dimensions by the ingenious devices and methods they have worked out.

On the contrary, Mr. Metcalf and others, reasoning from analogy, say that cast-iron and steel, being the same metal differently alloyed, but behaving essentially the same under the same conditions, the method which was so successful with the one is at least worthy of a trial with the other.

Were this reasoning advanced by a novice we might possibly hesitate; but when it comes from one whose experience with gun-casting is beyond that of perhaps any living man, and who is at the same time an acknowledged expert in the manufacture of steel, its rejection without fair trial can only be set down to the account of prejudice.

Situated as the authorities of the United States are, with the whole subject before them, it is inconceivable that they should be willing to launch the country upon the sea of expense that will be inevitable if a system of built-up guns be adopted without incurring first the very moderate expense that will be necessary for a thorough test of the Rodman system as applied to steel.

Theories in this case should give way to facts. The conditions are too complex to be theorized upon except in the most general way; and the engineering profession can only look on until the main facts have

been made clear by experiment. So far as we now know from the experiments on built-up guns, uniformity of production has not by any means been reached, and there have been some lamentable failures. Mr. Metcalf points to the uniform success met with in the production of cast-iron guns, and claims that the manipulations necessary for a similar production in cast-steel are well understood and not difficult.

There is a substantial agreement as to what is required in the ideal gun; let us not follow in the beaten track as copyists, if by a bolder and truly American course we can do better by following up our own traditions. The result is worthy the effort.

Mr. JAQUES.—I do not know to what particular guns Mr. Collingwood refers, but as the accident to the English Collingwood gun has been given great prominence, I ask your attention to the fact that this gun was not all steel, but built of an iron jacket and steel tube of low grade, which tube was not hooped to the muzzle. Of the material of this gun the investigating committee reported: "The metal was irregular in its character. The metal had not been subjected to annealing processes, annealing not having been adopted until a date subsequent to the manufacture of this gun."

I ask also your consideration of an extract from my letter to the New York Chamber of Commerce, dated February 2d, 1887, as follows: In regard to the reported failures of modern steel guns, I assert that there never has been an explosion of a modern high-power, forged steel, built-up gun of the material and system of construction recommended by the Gun Foundry Board in 1883, and now being manufactured by the Naval Bureau of Ordnance.

Mr. JOSEPH M. KNAP, M. Am. Soc. C. E.—I agree with the author of the paper under discussion that we have not yet arrived at the solution of the heavy gun problem, and coincide with his views that the best heavy gun of the future will be that known as the steel cast gun.

As regards smaller guns, say from 8-inch caliber and less, such satisfactory results have been obtained both in Europe and in this country with built-up guns made of forged steel, that our Government should be encouraged in their efforts in this direction.

While I would stand second to none in my admiration and appreciation of the late General Rodman, both as a scientist and an eminently practical ordnance officer and inventor, I must say that while he gave us the best heavy gun of his day, and thus placed us a long way in advance of all other nations in the matter of heavy ordnance, that is no reason why we as Americans should decry everything European, and shut our eyes to the fact that during the last twenty years other nations have made long strides in advance of us.

The essayist has given us the bright side of the Rodman cast-iron gun; it had its dark side as well. Much has been said of the treacherous nature of steel, more particularly that of high steel. What metal is not

treacherous? Cast-iron certainly is. I well remember, while associated with Mr. Metcalf in the manufacture of guns at Fort Pitt Foundry, our losing two or three fifteen-inch Rodman guns by their breaking in the mould. In each case the result was due to bad manipulation—carelessness in preparation of the small hollow core leading through the sinking head, by which a portion of the water was allowed to trickle down between the mould and the surface of the gun, chilling and weakening that portion to such an extent that it could not withstand the shrinkage strain. The metal and treatment in the cases of these cracked guns were the same as usual. No doubt had a weaker and softer iron been used, the rupture would not have occurred; but General Rodman did not lower his limit of density and tenacity on that account, for he held that the higher the density and tensile strength of the metal consistent with safety as regards shrinkage strain, the stronger and more enduring would be the gun. It should be so, in my opinion, in regard to steel guns. Mr. Dorsey, in his paper on "Steel for Heavy Guns," recently read before the U. S. Naval Institute, advocates the use of "mild" steel with a very low percentage of carbon, and having a tensile strength of 55 600 to 65 000 pounds, his main argument being that such steel is not so liable to break by the application of sudden shocks as steel of a higher density and tenacity.

Why not go back to soft cast-iron guns and done with it, if hardness and tensile strength and elasticity are not requisites in a gun?

One advantage gained by General Rodman's method was the securing of a higher density and increased tenacity of the metal near the bore, where the strain due to impact and wear due to abrasion are the greatest.

The history of the manufacture of the heavy cast-iron guns in this country shows that a constant increase in the density and tensile strength of the metal led to a corresponding improvement in the guns, both as regards their endurance and reliability.

Of course there was a limit, and there is doubtless such a limit in steel. Nothing but practice and careful experimenting will decide where that limit is; but there have been enough both in this country and in Europe to show us that very reliable serviceable guns of medium caliber may be constructed of forged steel having a tensile strength of from 90 000 to 100 000 pounds, and elastic limit of 45 000 to 50 000 pounds. Now, while I would advocate the construction of such guns without delay for our army and navy, it seems to me that Congress should be urged to appropriate a sufficient sum for the construction of a cast-steel gun of large caliber—say 12 inches bore and 50 tons weight, to throw a projectile weighing about one thousand pounds.

The necessary additional cost of plant to that already available in private establishments in the United States would be trifling compared to the outlay which would be required for heavy hammers, etc., for the construction of a built-up gun of the same size; and the cost of manufacture would be far less.

Too much credit cannot be given to the ordnance officers of our army and navy who, in spite of limited appropriations, have brought our steel ordnance up to its present high standard of efficiency; and whether the Rodman of the steel gun era is evolved from the army, the navy or from civil life, let us not fail to recognize the admirable services rendered by the "Gun Foundry and Fortification" Boards, and the "Select Committee on Ordnance and War Ships," nor to appreciate the painstaking, laborious and intelligent work of our friends in the ordnance corps in both branches of the service.

Mr. A. H. EMERY.—I did not come here this evening with the intention of making any extended remarks on the subject of ordnance, but I would say that it is a subject to which I have given a great deal of thought and upon which I have spent considerable money. I have also spent several years of my life in that kind of work, which has given me a very considerable knowledge of materials.

In regard to the paper by Mr. Metcalf, I have read it with a great deal of pleasure. I visited the foundry during the construction of the first 15-inch gun that was ever made; it was a great gun at that time. The charge was 30 pounds of pebble powder with a 300-pound shell. This was afterwards increased to 50 pounds. Three 20-inch guns have since been made on the same plan; the 20-inch bearing a charge of 200 pounds of coarse powder, with a 1 000-pound projectile. We require to-day not 300 pounds of metal for a projectile for the 15-inch gun, but 1 600; and not a velocity of 1 400 feet, but at least 2 000 feet at the muzzle of the gun. It is believed by engineers who have given much thought to the subject of cast-iron, and the action which occurs in the interior, that a metal with a tensile strength of 30 000 to 33 000 pounds, no matter how uniform, cannot endure those strains which must come upon the interior of the gun to give this high velocity to such heavy projectiles. I know there are some interested parties who are willing to risk money in the belief that such a velocity can be reached.

Mr. Hunt, of the South Boston Iron Foundry, has cast a 12-inch rifle which weighs about one hundred and eight thousand pounds, which has succeeded in giving a projectile of 800 pounds weight a velocity of 1 750 feet; that is a very great degree of success.

Mr. WM. J. MCALPINE, Past President Am. Soc. C. E.—Was that a cast-iron gun?

Mr. EMERY.—Yes, sir; a cast-iron gun. There is no doubt in my mind that Mr. Hunt can repeat that gun a hundred times and it will be just as good as the first one. Mr. Hunt is of the belief that he can make those guns of the same length that we propose to make the steel guns, which will be able to increase the velocity of the same projectile (800 pounds) to 2 000 feet per second. My own opinion is that he will be disappointed if he makes the effort; that the additional strain upon

the gun will burst it before it has had many firings. I have no doubt that such a gun would stand firing a number of times with that velocity, but not anything like 200 rounds.

The cast-iron gun, I will here say, has one element of value and strength which many builders of steel guns have not properly considered, and that is the ability of cast-iron to bear high strains of compression without being crushed. It is true cast-iron yields under strains of compression with a low pressure past its limit of elasticity, but when this metal has passed its limit of elasticity, the yielding is not so great but that it transmits this pressure to the walls of the gun without flowing.

An experiment was made at the Watertown Arsenal with some cast-iron cylinders. They were bored out and lined with wrought iron, as the interior of the guns had been bored and lined. The wrought-iron had a limit of elasticity at twenty-two thousand to twenty-three thousand pounds to the square inch; the cast-iron had a limit of elasticity at less than fifteen thousand pounds. These cylinders were all 11 inches in diameter outside, and when finished, 3.3 inches inside; one set was bored out to 3.5 inches and lined with .10-inch copper; another set was bored out 5.1 inches to receive a wrought-iron lining .9 inches thick. Now we have $\frac{9}{10}$-inch of wrought-iron lining in one set to offset $\frac{1}{10}$-inch of copper and $\frac{8}{10}$-inch of cast-iron in the other set; that is to say, the 11-inch exterior has a 3.5-inch bore in each case, but we have replaced the interior walls of one set by $\frac{9}{10}$ of an inch of wrought-iron to take the place of $\frac{8}{10}$-inch cast-iron and $\frac{1}{10}$-inch of copper in the other set.

But on being tested under the pressure of bees-wax on the interior, the set which was bored out and lined with wrought-iron burst with much lower pressure than did the set which was lined with $\frac{1}{10}$ inch of copper, because the wrought-iron flowed with much lower pressure than did the cast-iron which it replaced. The pressure in bursting the cast-iron cylinder was something exceeeding 90 000 pounds per square inch. The wrought-iron commenced flowing with less than 30 000; it was flowing very considerably with 40 000, and rapidly with 50 000 to the square inch, and was then aiding the wax to burst the walls around it. The cast-iron did not flow on the interior but slightly by that pressure which burst the cylinder, hence the ability to bear a much greater pressure before bursting.

Now if we take the same considerations and pass to the soft steel gun, we will certainly find that if we undertake to produce large pressures in the bore and hold them within the walls, that the interior walls will flow and try to burst the exterior.

In regard to making a gun of one piece of cast-steel annealed, I would say that some years since I was at Pittsburgh and visited the works in which Mr. Metcalf is interested, and was pleased to see a forg-

ing machine taking cold bars of steel and reducing the bars very considerably; it passed through the machines again and again cold, being reduced in diameter by the hammers each time. I was very much impressed by the reducing of this bar of cold steel by the rotating hammers. I have had occasion to use a number of those bars. I must say that they are very fine steel, better, I think, than casting can possibly be.

I think in regard to the low cost of the solid cast gun (I mean by that a steel gun cast in one piece and cooled from the interior) that we shall not find the cost low, but the quality may be. I do not expect to live until we can make a large mass of steel as good as the small masses have been made. As regards the cost, I agree with Mr. Sellers, who says in regard to making cast guns in one piece that if he were to do it he would require a greater price than for the built-up gun. He would consider the difficulty of getting a gun of good quality so great, so many of them would have to be condemned with the balance all uncertain, that he would greatly prefer to forge the gun in pieces and bore and assemble them.

As to what the gun of the future will be, I may say briefly this—built-up gun of steel—and I would say that my experience as an engineer teaches me that the smaller the mass we finish the pieces of steel in, working it by the hammer or hydraulic forging, the smaller the mass the better we shall find its quality; the smaller the mass, the better we can inspect it and know whether it is good.

I want to digress from Mr. Metcalf's paper so far as to answer one point in a paper presented by Mr. Dorsey before the U. S. Naval Institute, in which he stated that no engineer would put high steel to a structural use.

There was one place where I could not put a large mass of metal, this was a pin to go through the bars which had to be broken. I was obliged to keep the diameter of the pin which endures the whole load to 6 inches; and in order that I might get a piece of steel strong enough to stand this without being crushed, that would endure the shocks with safety, I used a piece of steel so hard that the men who dressed it in the lathes stated that they had great difficulty in drilling the centers. That steel has been in place for eight years and appears to be as strong to-day as the day it left the lathes; the pins are, so far as I last heard, entirely uninjured, so that some engineers would trust high steel.

Mr. EMERY (in reply to a question).—If there is no initial strain in the metals, then the question will be determined very largely by the length of the barrel. If the section loaded is very short, the solid ring will bear considerably more than the two rings, and if the section loaded is very long there will be no difference whatever.

Mr. THEODORE COOPER, M. Am. Soc. C. E.—What do you mean by very long?

Mr. EMERY.—I mean to say ten or fifteen diameters. If the whole interior is loaded and the tube is not more than one caliber thick and the length of the tube was ten or more diameters, so that it is loaded through its whole length, there would be no difference between bursting the one with a solid wall or the one where two or more tubes of the same quality were used to make walls of the same thickness.

Mr. COOPER.—Is that based on a test?

Mr. EMERY.—It is a matter of fact.

Mr. COOPER.—You have experiments bearing that out?

Mr. EMERY.—Yes, sir; it is perfectly immaterial whether the wall of a given thickness is made of 50 or 100 tubes, so long as the quality is the same and there are no initial strains.

Mr. COOPER.—Have you tests showing that?

Mr. EMERY.—No; but these are the facts of the case.

Mr. COOPER.—It is not a fact but an opinion.

Mr. EMERY.—If the section loaded on the interior has the same amount of compression on the loading material, the amount of tension with the same quality of metal will be the same on the walls with the thin tubes as with the thick tube; and it is immaterial whether that is a pound or 50 000 to the square inch until the limit of elasticity is reached, and then I see little or no difference to be expected until after more distortion than would ruin the green tube.

Mr. COOPER.—I don't agree with you.

Mr. EMERY.—When several thicknesses of tubes are put together I don't know any difference between the strength of several and the strength of one of equal thickness and quality; but in the case of thick walls like these, the interior tube must break first, because the strain is greater than on the exterior.

Mr. JOHN COFFIN.—In order to make what I shall say intelligible, I will preface my opinions by referring somewhat extendedly to an article written by J. A. Brinell, which is, in my opinion, the leading one of the age upon this subject. It was published in the first number for 1885 of *Jern Kontoret's Annaler*, in *Stahl und Eisen* for November, 1885, and in "Notes on the Construction of Ordnance, No. 37," printed June 22d, 1886. The *Stahl und Eisen* edition is exhausted, but *Jern Kontoret's Annaler* may still be obtained, and is very valuable for its excellent plates, which are the best I have ever seen.

Believing, in this case, an understanding of the subject will be more easily reached if the ordinary course is reversed, I repeat the Summary of Conclusions first, after explaining that by "hardening" carbon is meant that form of carbon found in hardened steel (whatever it may be), and by "cement" carbon that form of carbon found in annealed steel.

Summary of Conclusions.

First.—If steel loses its coarse crystalline structure without mechanical treatment, the change of structure must always be accompanied by the change of "cement" carbon to "hardening" carbon, or *vice versa*, and the cause of the breaking up of a coarse crystalline structure is always the change of carbon.

Second.—A coarse crystalline structure is entirely broken up only when the change of carbon is caused by heating; the coarse crystalline structure of either hardened or unhardened steel being completely broken up as soon as the temperature just reaches the point necessary to cause the change from "cement" to "hardening" carbon.

Third.—Steel which has been heated white hot must, if it is desired to change its carbon to the cement form, be cooled slowly to a temperature below that at which "cement" carbon is changed to "hardening" carbon while heating.

Fourth.—The change from "cement" carbon to "hardening" carbon is very rapid as soon as the temperature has reached the proper point. On the contrary, "hardening" carbon changes to "cement" carbon very gradually.

Fifth.—The change from "hardening" carbon to "cement" carbon is always accompanied by a production of heat, and it seems probable therefore that the change from "cement" carbon to "hardening" carbon is accompanied by an absorption of heat.

Seventh.—Rapid cooling never gives an amorphous or fine crystalline structure in steel which was of a coarse crystalline structure just before quenching; but it prevents a steel which was amorphous, or melted just before quenching from becoming crystalline during the cooling. In other words, rapid cooling fixes the structure which the steel possessed before quenching.

Eighth.—Change of carbon from the "hardening" to the "cement" form requires not only a correct temperature, but a certain amount of time; while, on the contrary, the change from the "cement" to the "hardening" form depends only on the temperature; and quenching seems consequently to prevent any change from "hardening" to "cement" form.

Ninth.—Crystallization also requires time, as well as a certain temperature; and if the time of cooling is shortened by quenching in water or by other means, the formation of crystals is limited or entirely prevented.

I omit conclusion sixth, thinking the experiments cited do not verify it, and am tempted to believe it is a misprint, or that the true meaning is obscured in the translation, as numerous experiments which I have made seem to refute it.

I think conclusion third would be a better deduction from the experiments, and a fuller statement of it if written: "Steel, while at a heat above that at which its carbon was changed to 'hardening' carbon while heating, must, if it is desired to change its carbon to 'cement' form, be cooled to a temperature below that at which 'cement' carbon is changed to 'hardening' carbon."

I have given the foregoing conclusions in full, for I believe them to be correctly drawn, having repeated about eighty of the experiments

given, and tried many original ones; and although I have formed opinions differing from his in some instances, I indorse the majority of his statements.

Brinell uses the term "cement" carbon probably in deference to Akerman and Rinman, but until chemists agree more fully upon the question of the chemical relation of carbon to steel, and while theories of solutions, combinations and alloys are at war with each other, I prefer the much more expressive and simple term "non-hardening" carbon, and hereafter I will use this expression to indicate that kind of carbon found to be the greatest part of the carbon in annealed steel.

I append an illustration, Plate XXII, copied from Brinell's article, showing the appearance in the dark, of a flat bar of tool steel while cooling, after being heated at one end to a bright orange.

One might judge, while performing this experiment, that during the time of cooling (or at some time in its transition from the hot to the cold state) it was slightly reheated, since portions that a moment before were colder than other portions, become hotter even when more exposed to cooling influences. Opinions based on this fact alone are not however conclusive, for the measure of the heat by the eye being only a comparative one, the fact that the relative heat of different portions of the bar changes, does not prove that the colder portions becomes any hotter, for this result might have been produced by the hotter portion growing very much colder. But the order in which these changes occur, leads us to believe that the steel is actually self-heating.

The first and most noticeable change of appearance is the formation and subsequent widening (in one direction only) of a bright band located between the hot and cold portions of the bar. Accepting the theory of the reheating of this band being caused by the change of carbon, we would infer that the first appearance of the band indicates the point of lowest temperature at which the carbon was in the hardening form before cooling; and as the point of lowest temperature of the hardening carbon portion of the bar is that point lying next to the non-hardening carbon portion, it follows that the bright band is the boundary line between the different forms of carbon.

I have tried many hardening experiments to prove this fact. Referring to Fig. 5, you will see a dark spot in the center of the bar, which, if this theory is correct, would indicate that in this place the carbon has not been changed to the non-hardening form, and if the piece be cooled in water the spot will be hard. Such we find to be the fact, anomalous as it may seem.

Another point in the observation of the cooling bar tends to strengthen conclusion third; the bright band does not appear until some time has elapsed and the bar has cooled somewhat.

To prove beyond a reasonable doubt that there really was a self-heating, and a consequent expansion, I tried a contraction experiment,

and found that a bar of .90 per cent. carbon, four feet long, at one time during its transition from a hot to a cold state expanded $\frac{1}{32}$ of an inch.

I append Plate XXIII, a somewhat abridged copy of Brinell's chart, which furnishes us with graphic illustrations of the experiments for determining changes in the structure of steel. The horizontal lines indicate different degrees of heat, the lower lines representing cold or black heat, the upper ones white heat. *W* is the most suitable hardening temperature, or the refining temperature. *V* is the temperature at which the principal change from hardening carbon to non-hardening carbon commences, and is between red and low red in the dark. The vertical lines show the way in which the experiment was performed, the light lines meaning slow heating or cooling, and the heavy lines sudden cooling in water. In all cases the slow cooling was done by imbedding the steel in dry coal dust, if possible; otherwise it was allowed to cool in the open air. Small circles indicate the starting point, arrow heads the end of the experiment.

The eighty-two experiments were made with steel from the same Bessemer ingot, the chemical composition of which was as follows:

Carbon	.502
Manganese	.48
Silicon	.13
Sulphur	.Trace.
Phosphorus	.026

The letter underneath refers to the type of fracture, a composite fracture being indicated by two letters, and the following are the descriptions thereof.

A.—Coarse-pointed crystalline. The large crystalline surfaces shining. Color of fracture tending to blue.

B.—Pointed crystalline. Surface of crystals bright. Color of fracture tending to blue.

C.—Fine-pointed crystalline. Crystals small and bright. Color bluish.

D.—Coarse granular. The large crystal surfaces of a silvery color. Color of fracture white.

E.—Fine granular crystalline. The small crystal surfaces silver-white.

F.—Light amorphous. No plain crystallization can be seen by the naked eye.

G.—Flaky crystalline. Surfaces of crystals bright. Color of fracture somewhat bluish.

H.—Dark amorphous. Fracture soft and dull with no crystallization.

I.—Large coarse crystalline. Crystal surfaces looking like unpolished silver.

The treatment of the steel before the experiments was as follows:

Group I.—Finished forged at a red heat and cooled slowly. Fracture *C.*

Group II.—Heated white hot and then cooled slowly in dry coal dust. Fracture *A.*

Group III.—Heated white hot and then quenched in water. Fracture *D*.

Group IV.—Heated to temperature *W* and then quenched in water. Fracture *F*.

Group V.—The same as IV. Fracture *F*.

Group VI.—The same as I. Fracture *C*.

Group VII.—The same as I. Fracture *C*.

Group VIII.—The same as I. Fracture *C*.

Group IX.—The same as I. Fracture *C*.

Group X.—Melted.

I have placed charts before you and explained them thus exhaustively, because I wish to refer to some of the experiments in my subsequent remarks; and the article from which they are copied has never, to my knowledge, been published in English, except as an ordnance note, which had a limited circulation. The *Iron Age* published the conclusions some time ago.

Annealing Steel.

Annealing steel may be properly treated under three heads:

First.—Changing its carbon.

Second.—Removing its internal strains.

Third.—Changing its structure, or refining it.

If a piece of steel is hardened and then slowly heated, its carbon begins to change to the non-hardening form at a very low temperature. Not having studied the changes which occur when it is heated below a bright straw color, I am not prepared to say there are any, though I am inclined to think some take place.

In my examination of Brinell's acid test for hardening carbon, I made the following experiment with twelve pieces of tool steel cut from the same bar, each piece being one inch square and a quarter of an inch in thickness. I first numbered them on one side, and hardened them uniformly, then polished the unmarked side and annealed them (drew the temper) as follows:

No. 1, I left hard; No. 2, I made a very faint straw color; No. 3, a darker straw color, and so on up to No. 10, which I made an ash-gray color. No. 11 showed a very little red in a dark room, and No. 10 was a full red, or as soft as I could make it. I then repolished the surfaces, and placed on each a drop of nitric acid with specific gravity 1.23, leaving it for 45 seconds before washing in running water. I then arranged them in order by examining the colors, and, on turning them over I found I had followed the numerical order. There was a good deal of difficulty in distinguishing the dead hard one from the light straw color, and also with regard to the three which had been made hottest. No. 1 was a brownish-black, and No. 12 a bluish-gray. The reason of these different shades of color I do not know, but that the variation is caused by the fact of the carbon in each case being in a different form, seems very likely, since a piece of hardened steel of low carbon gives the same

blackish color, even though it may be softer than No. 10 of the above series.

Although the comparison of the different kinds of carbon by this test seems to end practically at a blue heat, beyond this point drillings may be readily made, and direct chemical tests be tried.

The elastic limit of a tensile specimen is a good comparative test where the elastic limit of the same steel, having the same structure, is known when fully annealed.

The carbon is not all changed to non-hardening carbon; or, in other words, the temper is not all drawn out until a low but distinct red is reached. In all steel ranging from 20 per cent. up, my experiments have tended to prove this statement true, and though I had never experimented on boiler-plate stock, I intend to do so as soon as I am prepared for it.

Mr. Metcalf says: "The recent United States Navy specifications would read better if they said to be annealed, hardened in oil, and have all the temper drawn out."

Perhaps they might read better, but they would call for a quality of steel never before used in guns, as it is not customary anywhere in the world to draw the temper all out for this purpose.

We have been making gun steel under the following specifications: Elastic limit, 55 000 pounds; elongation 12 per cent., in a 6-inch test piece; and a piece with the temper all drawn out gave elastic limit 44 000 pounds; ultimate, 86 150 pounds; elongation, 25 per cent.

The fact is our gun steel is tempered steel, and I think, from numerous experiments, the temper is not all drawn out until a temperature is reached corresponding to *V* on Brinell's chart. I can see no reason for a connection between the temperature at which carbon begins to change to non-hardening carbon while cooling, and the one at which the change ends while heating, but they seem to be one and the same. The influence of time on the change to non-hardening carbon appears to be limited after the lapse of about two hours, since at a regular low heat all the carbon that can possibly change at that temperature seems to change at that time, and if it is desired to make the steel softer, the heat must be increased. If in drawing the temper of a gun hoop we fail to get it soft enough, repeated heating to the same temperature seems to have no effect on it, it being necessary to raise the temperature in order to draw the temper more.

Internal strains are caused by unequal cooling after casting; by hammering at a cool heat; by tempering; or by unequal cooling of any kind. Whatever may have caused them, however, they may always be removed by annealing the steel properly, and I will endeavor to show that those caused by tempering, though alike in their effect, can be removed at a lower heat.

I have discovered that while the carbon is changing from hardening

to non-hardening, or *vice versa*, the steel is very weak and easily bent, and I will illustrate the fact by the following beautiful experiments: I took a bar of steel whose length and section were sufficient to sustain its weight when it was supported at the ends in a furnace at a temperature above *W*. This bar was heated to a temperature greater than *W* while lying on the level furnace bottom, then raised and supports placed under the ends. After half an hour in this position it had not deflected an appreciable amount, but on being taken from the furnace and allowed to cool while similarly supported, as soon as temperature *V* was reached its downward motion was so rapid as to be readily seen by the eye.

Being then straightened, its behavior was observed while heating on the supports, and it was found to pass temperature *V* without any deflection, but at temperature *W* it bent down as it had before done at temperature *V* while cooling; in both cases it bent while the carbon was changing form. To discover if the same weakness occurred while the carbon was changing to the non-hardening form during the drawing of the temper of hardened pieces, I performed the following experiment. In Plate XXIV, *A* and *B* were pieces of half inch square steel, five inches long, cut from the same bar, and filed as shown, leaving bearing places at *c*, *d* and *e* a trifle higher than the rest of the bar. Care was taken to have the bars of exactly the same section, and they were then uniformly hardened, and the temper drawn to a bright blue on the one marked *A*. The bearing places *c*, *d* and *e* on both bars were then ground and lapped until they fitted on a surface plate, the center spot *d* on both being left an appreciable degree higher, so that when pushed on the end they would turn on the middle, although light could not be seen under the ends. They were then polished on the sides and clamped together at the ends, as shown, with a very thin strip of metal separating them in the middle. The strain was not severe, for after one clamp was put on the other ends could be easily held together with the fingers. They were then heated slowly and uniformly to a very light straw color and allowed to cool slowly. On *A* there was no change of carbon during this heating, for it had previously been heated to a blue. I found it still revolved on the center, and was in exactly the same condition, as far as I could determine, as before. On *B* there was a change of carbon during the heating, as I found it was bent to such an extent that it revolved on the ends and light could be seen under the middle, but only a very small portion of the tension put on by the clamps was removed.

I have formed the conclusion that if there is a change of carbon during annealing, internal strains are removed at a lower heat; but from the detail record of physical tests, verified by experiment on rings put under known tension and heated, I have concluded that all internal strains were removed below *V*. When steel is heated to remove its coarse crystalline structure, it is necessary to fully understand the effect of heating to achieve the best results. You will see by referring to

Group II, Brinell's experiment, where a steel was previously heated high and cooled slowly, there was no change in reheating experiments 1, 2 and 3 both giving fracture *A*, but as soon as temperature *W* was reached, in experiment 4, we have a different fracture. The same result is observed in experiments 7, 8, 9 and 10. Comparing experiments 4 and 10, in which the same temperature was attained, we find that 10 gives the amorphous fracture *F*, while 4 gives the pointed crystalline fracture *C*. It appears evident that all these crystals were formed while cooling slowly from *W*, and if you will refer to Group V, you will see that no crystallization took place below the temperature *V*; so it is fair to presume that in experiment 4, Group II, all the crystallization took place between the temperature *W* and *V*. I see no reason why crystallization should take place only when the carbon in steel is in the hardening form, but such seems to be the case.

Another fact not shown by these experiments, but observed by myself in many cases, is that the slower steel is cooled from temperature *W* to temperature *V*, the coarser its crystalline structure will be. The steel bars on which these experiments were tried were about three-eighths of an inch thick; therefore as the time in passing between given temperatures must necessarily be short compared with the time required for a large forging to pass through the same range, the thought may suggest itself to some that if, for example in experiment 3, Group II, the hightest temperature shown had been maintained for several hours, the fracture would have been different. I have not found this to be true. Hundreds of examples present themselves to my mind in which such a temperature had been maintained for hours and even for days, without affecting the fracture. We broke the other day a large 5-foot steel pinion which had been running two years. This pinion, before putting in, had been annealed by building a small furnace around it and firing with coal. The annealing certainly benefited it very much, by taking out the internal strains, else it would not have given such good service, but the fracture of the broken tooth looked exactly like the fracture of an unannealed steel-casting. A steel roll was annealed in a pit by means of a wood fire being made around it, and after the desired heat was reached the pit was sealed up. I noticed that the temperature was not quite uniform, the top being a little cooler than the bottom. Now the effect of heating in this pit with a large mass of glowing charcoal and unburnt wood is to maintain the temperature for a long time, yet in dressing out the wobblers it was found that while the side that was down in the pit was fine-pointed crystalline, the top side gave the characteristic fracture of an unannealed casting. Since the above pieces were annealed, we have, I think, made some progress in the art of annealing steel castings. Our mode of operating now is to heat uniformly to the temperature *W*, and if the very best results are desired, we open the sides of the furnace and cool as rapidly as is possible by drafts of air until temperature *V* is reached, when we seal up the furnace and

cool slowly. The temperature *W* does not vary much for different grades of ste It seems to be modified more by the varying amounts of manganese and silicon than of carbon.

I am not prepared to state at present exactly what these effects are, but will say broadly that the temperature *W* is practically the same for all grades of steel we use, commencing at .25 per cent. carbon, Bessemer and open-hearth, up to 1.50 per cent. carbon, tool steel. You have before you two samples of steel, the one labeled *A* is 1.50 per cent. carbon and .14 per cent. manganese. The one labeled *B* is .50 per cent. carbon and .67 per cent manganese.

You will see by examination of the fractures they are refined about alike. They were heated to the same temperature as near as possible without putting them in a lead bath. They were heated side by side in a large charcoal fire with very little blast. When the temperature *W* was reached, they were quenched in water. Neither piece is very good steel; the soft one, you will observe has a piping, the hard one happens to be a poor piece of tool steel. I cannot give the reason.

While trying these experiments I thought I had a piece of .30 per cent. carbon, but afterwards found it to be .50.

Sample *C* is before you, to show the effect of refining three times a piece of steel which had been very much overheated. Perhaps twice would have been enough had I known it was .50 per cent. carbon.

It is shown by these experiments that th. act of changing carbon to hardening form causes a breaking up of crystals. We also see that there are important phenomena manifested when carbon changes to non-hardening form, and among these is the tendency to break up crystals, as shown by the interchange of crystal particles at the faces causing dull crystal faces, and it seems fair to suppose that the only reason why the crystals are not entirely broken up as in the other case, is because the temperature is not high enough. My experiments in this direction are not complete enough to warrant me in offering this, except as a mere conjecture; but if it be true, and I am of the belief that subsequent experiments will establish its truth, it follows that if steel is to be in any way refined by the change of carbon to non-hardening form, the higher the temperature at which this change takes place the better the refinement. I will illustrate. Suppose we have two pieces of steel under treatment. The first piece is heated up to *W*, and becomes for the moment amorphous. It is then quenched suddenly in water, and presents a fine bright surface, nearly amorphous as seen by the naked eye. It is then heated nearly up to *V*, or at a temperature high enough for all its carbon to pass into the non-hardening form; this change of carbon causes a partial destruction of the small crystals, so that under a very strong glass it may appear amorphous, and the color of the fracture is now much darker than before, showing that there are fewer reflecting faces. The second piece under consideration is heated in the same way to *W*, cooled rapidly to *V* and then cooled slowly.

We have the steel then in exactly the same conditions as regards its carbon, and these conditions were achieved at a different temperature; that is, the carbon of the first piece is changed at the lower limit of its temperature of change, and that of the second piece at its higher. The sample I submit to you (marked *G*) was treated by the second method. Under an achromatic triplet of high power I can detect no crystal forms; and though I recognize the term amorphous as applied to steel only a comparative one, this sample, I think, is fully entitled to the distinction. Steel treated in this way is very tough indeed. This piece had sharp *V* grooves planed across, as shown, before treatment, yet it was so tough that it bent through an angle of at least forty-five degrees from its original position before breaking. It was from the same bar of .50 per cent. carbon steel as the other fracture tests.

In comparison with this fracture and the others obtained by sudden cooling alone from temperature *W*, I present the specimen *D* obtained from the same bar of steel, by heating to the temperature *W* and slowly cooling in hot ashes. The character and strength of this form of crystallization is enough of itself to furnish matter for a long discussion, but I will not enter into it now, except by calling your attention to samples *E* and *F*, *E* being from a properly annealed casting, and *F* from an unannealed one.

The conclusions we have reached bearing on the subject under discussion may be summarized as follows:

First.—Steel must in any case be heated to temperature *W* to break up its coarse crystalline structure, a lower temperature continued for a long time having little or no effect upon it.

Second.—The more rapidly steel is cooled from temperature *W* to temperature *V*, the less crystallization will take place, and the stronger the resulting structure will be.

Third.—The slower steel is cooled from temperature *V* the more carbon will pass into the non-hardening form, and the greater interchange of molecules will occur between the crystal faces, giving them more cohesion, and, other things being equal, making the steel more tough and ductile.

Fourth.—Internal strains are removed at a lower temperature in steel which has its carbon in the hardening form before heating.

Fifth.—All, or very nearly all, internal strains are removed in any case by heating to temperature *V*.

Considering the question, Can we make a Rodman gun of steel such as we now use for steel castings? It is necessary to consider the definition of a Rodman gun. If a Rodman gun is a cast gun, having its initial tension developed by unequal cooling from the molten state in the mould, I answer positively, no, for steel castings have coarse crystals, strong in themselves, but weak in their union; and cooled rapidly from one direction, prismatic crystals are formed with their axes normal to the cooling surfaces (see sample), having very little cohesion between their side faces. These crystals cannot be broken up without reheating to tem-

perature W, and before temperature W is reached the initial tension is lost.

If a Rodman gun is a cast gun, in which the initial tension is attained by some heating and cooling method other than shrinkage of separate hoops, the question broadens itself out into new fields and becomes one of elastic limit, or material accuracy of manipulation and certainty of results. If anything is ever accomplished in this direction, the imperative steps of the process will be casting with the first cooling portions down; cooling evenly to prevent internal strains severe enough to cause automatic rupture, or the starting of such; reheating to temperature W, and cooling suddenly enough to temperature V to prevent a weak crystal formation; and finally, attaining the desired initial tension by heating and unequally cooling below temperature V. The difficulties in the way are great. A very large piece of varying section; uncertainty of maintaining desired temperatures throughout; a limited knowledge of the effect of these temperatures; and the great cost of all experimental work in this direction, lead me to believe that the day is very far distant for Pittsburgh's dream to be realized.

I am a patriotic believer in American talent and enterprise, and if solid steel gun construction is ever successfully accomplished, I would like to have the honors here, but in our struggle to again gain the front let us not like the crawfish go backward.

Commander C. F. Goodrich, U. S. N.—In common with other officers who have been more or less identified with the new guns, I confess to a confidence in them which I believe to be justified by the firings at the proving ground. Nevertheless the naval service is not, I trust, bigoted, and if Mr. Metcalf or any other person can give it weapons as efficient, as light, and as safe as those now coming into use, with the added merit of greater cheapness, I am sure they will be welcomed.

It is hardly worth while to record my doubts of the entire success of the proposed scheme. Such a procedure would be equivalent to prejudging a case at law already on trial in the courts.

The last naval appropriation provides for the building of three cast-steel 6-inch cannon; and their behavior in practice will settle the question more fully than is possible by any amount of discussion, however intelligent and honest.

In conclusion, let me say that the interest manifested by the engineering community in this subject of such vital importance is full of promise to us naval officers.

Lieut.-Commander F. M. Barber, U. S. N.—The well-known reputation of Mr. Metcalf entitles his paper, so far as it relates to heavy guns, to more consideration than that of any other metallurgist in the United States, because he is the only practical expert who takes up squarely the position that modern high-powered guns of any size can

be made of steel, cast in one piece on the Rodman principle. I do not value the favorable opinions expressed by non-practical men, such as army and navy officers, in a matter of this kind, because we do not have the shop experience, and outside of the mill the only method of obtaining information is by reading, inquiry and study, and the more one searches the more he becomes convinced that there is no information whatever on the subject of hollow-casting steel guns.

I have had access to the information on file both in the War and Navy Departments (and I know of no other source of equal value), and there is nothing that I can find which affords proof that any one has ever had success in casting whole unforged steel guns, either solid or hollow, except in field pieces. All the information is of a negative character and the opinions of writers are qualified. Mr. Wellman, of the Otis Works, for example, says in the Naval Institute discussion on steel for heavy guns.

"One thing has not to my knowledge been proved as yet, and that is that the high ductility that is asked for in hard gun steel is at all necessary. If this should prove not to be the case, then I see no reason why as good, if not a better gun, cannot be made by some modification of the Rodman process applied to a steel cast gun."

Mr. L. S. Burt, of Steelton, in a letter to Captain Michaelis, in 1884, thinks that they can be made, but states that the risks incurred in handling steel is much greater than that of handling iron, as it chills so quickly; and wants to know, if in the event of failure, would the loss fall on the contractors.

Mr. A. Pourcel, of Terre Noire, the most famous steel-casting establishment in the world), has, so far as known, never tried the Rodman principle on guns, but even with regard to large castings of any kind he says in his paper read before the Iron and Steel Institute at Vienna, in 1883 :

"But the final solution of this problem is still a long way off. The production of castings of any form and of any dimensions in steel of a well-determined chemical composition, combining the resistance and rigidity of steel with the smooth surface and homogenity of iron castings, is a very complicated problem and one which presents material difficulties of more than one kind."

At the time he was reading this paper, the most important work at Terre Noire was some hydraulic cylinders intended to stand a pressure of 10 000 pounds which were to be supplied to a Paris engineering firm. The physical characteristics were high and the test specimens gave excellent results. In the *Iron Age* of February 10, 1887, is recorded the fate of these cylinders; one of them burst at 1 000 pounds and the others were condemned. Four years ago Terre Noire had perfect success in casting small gun hoops. Large amounts of money have been expended there by the company and practical encouragement has been given by the French Government, but we cannot hear that they have yet advanced

from hoops to jackets, still less from jackets to tubes, to say nothing of complete guns. It is rumored that they are now in financial trouble. Still, as Mr. Metcalf says, they have never tried the Rodman principle. One wonders why they have not, as it has been an open secret for more than twenty-five years.

At Bofor's, in Sweden, they have succeeded in making cast-steel gun tubes up to 4.5 inches in diameter of bore (they use the Terre Noire process for getting solid castings), but the gun tubes are hooped with forged steel. No one since the date of Holley's book (1865) has produced better results than this, and yet the solid steel casting is only the starting point for a successful forged gun. It has not been found possible up to this time to make a modern high-powered forged steel gun except by commencing with perfectly flawless steel castings, either solid or hollow—for Whitworth patented the latter over twenty years ago—so small in proportion to the mechanical means employed, that each can be worked by mechanical means as well as by tempering and annealing, and each separate part submitted to inspection before the gun is put together. This is why the gun is necessarily built up. If it is unmechanical, as Mr. Metcalf says, there are some eleven obstinate jurors who disagree with him. If "definite shrinkage" is not a practical possibility, it is so near it that we get guns that are stronger than by any other process of fabrication yet practically established.

Mr. Metcalf thoroughly understands steel; he states in his paper more difficulties with it than I ever knew before. He points to the extrusion of foreign elements and the sinking by gravity when the metal cools; the variation of structure corresponding to the variation of temperature; the variation of specific gravity; the change in volume; the permanent internal strains arising from the two; the variation of the carbon; the cooling; the hardening; the annealing; the over-annealing; the non-fibrous, but crystalline character; the avoidance of angles and the marked effect of the chemical constitution. He states all these things with great clearness, and also specifies with equal lucidity what is sound doctrine as to the requirements of the metal of which a high-powered gun should be made, and then concludes by saying that he can take this very peculiar metal and cast a gun of any dimensions from it on the Rodman principle.

Like the gentleman whose opinion of his adversary was so high that he would not disagree with him even when he asserted two and two made five, I certainly am not prepared to dispute Mr. Metcalf's ability to do what he says he can; but his own enunciation of the peculiarities of the metal would seem to show why it is so difficult for other practical men to find financial success in making large steel castings where high requirements are demanded in the metal.

There is an apparent confusion regarding this matter of making cast-steel guns by the Rodman method. Mr. Metcalf describes the well-

known Rodman plan of chilling the interior of the bore by a circulation of water, and he says he would apply this to steel, but omits to say anything at this time of any after-treatment. Captain Michaelis (who Mr. Metcalf says "seems to be inspired by the spirit and courage of Rodman") states in his paper, read before your Society June 10th, 1884, that he proposes to carry out "a purely American idea—casting from open-hearth steel a Rodman gun annealed from the interior." Holley, who is the favorite authority on this subject, is quoted by Captain Michaelis as saying, "The casting of a piece which has the desired shape and requires no reheating beyond a slow annealing, is so great a progress that it must be obvious to all practical men, especially when it is considered that the product possesses in every part of its homogeneous mass all the physical qualities of forged steel." Holley wrote this at a time when he and every other intelligent man was profoundly impressed with the Terre Noire discovery, and he naturally prophesied a future for it which the lack of uniformity of its product has failed to verify. It should be noted that the remarks I have quoted from Mr. Purcel of Terre Noire, were made two years after Mr. Holley's death.

Now it is, I believe, the prevailing opinion that the Rodman method was designed to introduce initial strains in the casting, and so successful was it that, according to Mr. A. H. Emery (in his testimony before the Conference Committee on the Fortifications Bill, February 10th, 1887), a 15-inch Rodman gun, cast at Fort Pitt about 1863, and fired over one hundred rounds, showed seventeen years afterward strong evidence of its remarkable structure. The gun was condemned on account of scoring in the vent, and was cut up at the South Boston Iron-works some ten years ago. The first cut was taken in front of the trunnions, where the gun was 44 inches in diameter; the tool had gone into it but two inches when the gun cracked entirely through for three-fifths of the circumference with a considerable report, showing that it had been under a heavy strain all these years. Simpern's "Gunnery," published in 1861, says that two 8-inch guns were cast to test the endurance under fire. One was cast solid, the other hollow. The former endured but 73 rounds, the latter fired 1 500 rounds without bursting.

Now are Messrs. Rodman, Metcalf, Michaelis and Holley moving on the same line of thought, or are they not? Is the Rodman plan of casting alone a form of annealing? We ordinarily understand annealing to be a process intended to relieve a casting from internal strains, and if the gun is annealed from the interior after it is cooled, what becomes of the peculiar initial strains which constitute the Rodman principle; and if we continue this annealing until we reach what Holley admires, "that the product possesses in every part of its homogeneous mass all the physical qualities of forged steel" (which simply means uniformity), are we not still further away from the Rodman principle? If gun steel will cast on the Rodman principle, why is it necessary to anneal it after-

ward at all? I believe the popular superstition is that it chills and shrinks and swells to a greater degree and more irregularly than cast-iron, and does all kinds of things that it ought not to do. Whether this is because the carbon is chemically combined in the steel and mechanically in the cast-iron, according to the molecular theory, or whether the cement carbon turns to hardening carbon, or *vice versa*, whenever steel changes its crystalline structure without mechanical treatment, as asserted in *Stahl und Eisen*, November, 1885, I am unable to say. It is a case in which variety of theoretical information appears more likely to lead to confusion of ideas than to anything else. Still Mr. Metcalf has said elsewhere that if the first cooling were insufficient, subsequent heatings and coolings could be resorted to cheaply and effectively. It is an assertion from an able man, and we are now back to our first position. I am unable to dispute it.

Whatever may be the correctness of Mr. Metcalf's theory, private capital has up to this time been too timid to support it, and the result of the bidding on the navy gun contracts of March 22d, shows that capital can be found in ample quantities to support other ideas as to what the gun of the present is, whatever may be the gun of the future.

I sincerely hope, in connection with this matter, that the clause of the Navy bill appropriating $20 000 for successful cast-steel guns of 6-inch bore will cause Mr. Metcalf's theory to be as thoroughly tried as it can be with such small guns, and I am sure that there is no one in the navy but has the best wishes for his success. I think however that he is rather unkind in that portion of his paper pertaining to "modern writers scorning cost, and insinuating that civilians have not a dollar at stake in the gun question anyhow." This and the succeeding paragraph are calculated to give the impression that officers in authority are arrayed in violent opposition to his theory, and are therefore not only ignorant, but impracticable. The fact is, that so far as the officers in the Departments are concerned (and all other officers, so far as I am aware), there is no hostile feeling whatever. Up to the present time Mr. Metcalf and his followers have deliberately kept his theory in the same category with the theories of the hundreds of regularly recognized disciples of Colonel Sellers (Captain Michaelis asks for an annual appropriation of two millions in his paper), while the wrought-steel gun men have not done so. "The old flag and an appropriation" is no longer recognized by the committees in Congress as a sufficient reason for the expenditure of public money to demonstrate any one's theory. There is a vast difference between appropriating money to pay for a specified quality of goods delivered (the cost of production being at the expense of responsible manufacturers who will guarantee their work) and appropriating money to demonstrate a theory of manufacture and then appropriating more money to pay for the goods themselves if the theory should prove correct, and still more money to compensate the inventor, the Government to

lose the original outlay if the theory is not capable of demonstration. There is no question but the Government should pay handsomely for every idea and invention that it adopts, even if it pays the expense of investigation itself (though there are many officers who disagree with me on this latter point); but in this particular case what are we going to do for guns meantime, and what are the wrought-steel men going to say? Under the present condition of circumstances, in view of the enormous expenditure in prospect, the most serious question that Congress has had to solve is not how many kinds of guns shall we appropriate for, but which kind, and it is the disciples of Colonel Sellers who have succeeded in defeating the Fortifications Bill for two successive years by obscuring this point. They would defeat it next year also if Secretary Whitney had not fortunately solved the problem meantime.

I congratulate Mr. Metcalf on his happy association of officers with so respectable a portion of the community as the clergy. It is a compliment we do not often receive, but it does not appear that his covert sneer at religion adds much to the force of his judgment. I am sorry to say, however, on behalf of Mr. Metcalf, that our labors, either official or spiritual, are no longer to have the benefit of the advice of one who has been so long our guide, counsellor and friend, since he says in the closing paragraph of his testimony before the Senate Committee on Ordnance and War Ships, December 10th, 1884: "I never want to see another gun, so I write freely, wishing to give you all the information that is possible. The concern with which I am connected is entirely out of the line of such work and for myself I have had enough of it, and like to think of the gun business as belonging to the dead past."

There is only one point in Mr. Metcalf's paper which I particularly wish to criticise, and that is the statement that no Rodman gun ever failed. The statement is probably carelessly made, and may be intended only as a generality; but it is one that has been going round the country very generally during the past few months, and it should be corrected. In the report of the Joint Committee on Ordnance, Senate, page 215, Fiftieth Congress, third Session, there is a list of 249 cast-iron guns which have burst in the United States, mostly during the war. Of these 141 burst under fire, 10 burst spontaneously, and 98 cracked, fissured or ruptured before proof.

Those which failed under fire were as follows:

Parrott rifles	86
Rodman rifles	6
Dahlgren rifles	29
Rodman smooth bore	18
Dahlgren smooth bore	2

Of the 10 which burst spontaneously, 5 were Rodman and 5 were Dahlgren, and a circumstantial account is given of each case.

In conclusion, a few remarks about the late Mr. Holley's writings

may not be out of place in order to illustrate the absurdity of quoting him . . the advocate of Mr. Metcalf's theories. Holley's "Ordnance and Armor" was published in 1865. On page 417 he says:

"The soundness of steel castings, especially those produced by Naylor, Vickers & Co. (of England) and by the Bochum Company in Prussia, induced Captain Blakely to construct parts of some of his guns, such as outer jackets, of hollow ingots not forged but only annealed, and there is a growing impression in England that the heaviest ordnance will be cast solid from steel."

That is one for Mr. Metcalf. On page 46 he says:

"Messrs. Naylor, Vickers & Co. are perhaps more skilled than any other steel-makers, except the Bochum Company in Prussia, in the art of casting large masses of all shapes, such as tubes, bells, wheels, etc., sound and uniform throughout. It is considered however that the increase of strength by hammering will always warrant the expense of hammering in gun-work."

That is one for the wrought-steel men.

In his "General Conclusions on the Requirements of Guns" however, page 287, which it is fair to conclude are his own opinions, he says:

"On the whole a steel tube so tempered (probably by hardening in oil) as to have the greatest elongation within its elastic limits, and forced into (or otherwise compressed within) a heavy cast-iron jacket of good shape like the United States 15-inch hollow cast navy gun with trunnions and cascabel cast on for cheapness—the slight initial compression of the steel being sufficient to compensate for its want of safe elongation —would appear to be the best system of fabricating strong, cheap and trustworthy cannon of large caliber."

Now who is to find consolation at this day in that opinion?

I respectfully submit the following explanation as reasonable. When Mr. Holley was connected with the "Stevens' Battery," he was for a long time in England with very little to occupy his active, intelligent mind. He at this time collected the information so well expressed in his exceedingly valuable book. He himself was surprised at the popularity of the book, because he had only gone into the subject as an independent, unbiased, unprejudiced thinker, whose real business was entirely different from gun-making. It was undoubtedly this fairness of treatment which gave the book a value which the production of a hobby-horseman could never have equaled. But the book once finished, he lost all active interest in the subject (which the hobby-horseman would not have done), and his business, which was connected with the manufacture of Bessemer steel, fully occupying his mind, he never brought out a modern edition of his grand work. He was urged to do this not long before he died (in the spring of 1881), but he said that the gun and armor business had gone away beyond where it was in his day, and he had not kept up with the subject. It seems to me it is as fair to suppose that he would, if he were alive, be in line with the majority of other careful thinkers, as that he would be an advocate of cast-steel guns on the Rodman principle.

I question the value of the essayist's argument that the Rodman plan of casting should be applied to steel because it is "pre-eminently American." Why should Rodman be glorified at the expense of other American inventors? The success of his invention is undoubted; but it must not be forgotten that its value has been very much disputed even with cast-iron men, and long after the war was over Government made cast-iron guns at Fort Pitt, both solid and hollow for comparison. And in any event how can it be considered "pre-eminently American" when nearly all the other gun inventions now adopted in Europe are American in their origin also? The built-up gun itself is as much American as European. The best system of breech closure, now known as "the French" is undisputedly American. An American gas check is more used than any other in Europe. Slow burning powder is American. All the best machine guns and small arms are American, and so are the cartridges. Round forged projectiles, expansion bands, and rear fuzes are American, and I have been told on good authority that an old farmer from Ohio originated, over thirty years ago, the Whitworth system of rifling which has always given the greatest ranges to cannon.

All these inventions found a home abroad, and were developed there simply because the soil of Europe appears to be better adapted to the growth of bayonets than it is to plow-shares, while ours fortunately is not; but all the same the inventions are American and their reception in whole or in part in the land of their birth can hardly be deemed unpatriotic.

Mr. WILLIAM METCALF.—If Mr. Barber will read carefully the discussion on Mr. Dorsey's paper, he will find, written by some officer, a sneer at the opinions of those "who have not a dollar at stake."

I distinctly disavow any "covert sneer at religion," as my faith in Christianity is incomparably deeper than my faith in any guns. I did crack a joke at a well-known peculiarity of the members of the highest profession, but it was at the men and not at their principles.

Lieut. R. R. INGERSOLL, U. S. N.—I have read with very great interest Mr. Metcalf's paper on "steel," and have been much instructed by it. It is not my purpose to attempt a discussion of any part of the paper which does not relate to the finished gun. The best plan to obtain certain physical characteristics in gun steel belongs to the domain of manufacturers of steel, and while the various processes possess peculiar interest to the student of ordnance matters, yet there are none so competent to judge of the methods by which good gun steel is produced as those engaged in that work.

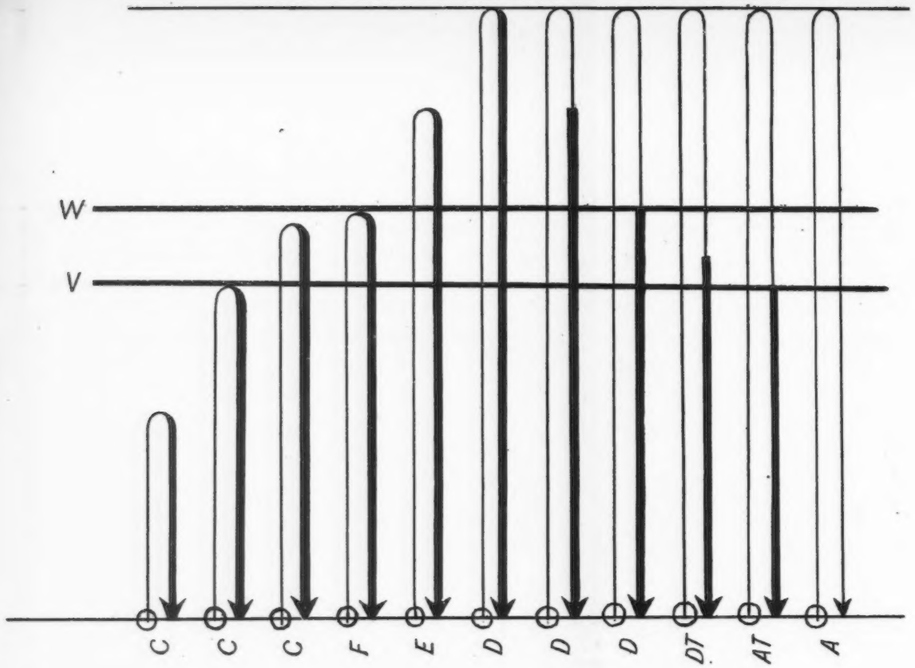
The condition in which the finished gun is delivered to the artillerist is however a legitimate topic for a naval officer to discuss. I regret that the lecturer gave but few facts concerning the condition and strength of the gun as it leaves the hands of the gun-founder.

No one will question the value of the Rodman process for producing a stronger cast-iron gun than can be obtained by any other method of construction with that metal. When applied to steel the natural conclusion would be that the process of hollow casting, if successfully accomplished, would give a stronger steel gun than by any other system of construction when steel of the same quality is used. Up to this point, the completion of the casting, there will be but little difference of opinion, but the steel cast gun is subjected to the process of annealing and it is in regard to the effect of this process on the elastic strength of the finished gun that I venture to ask the lecturer to give us a little more light.

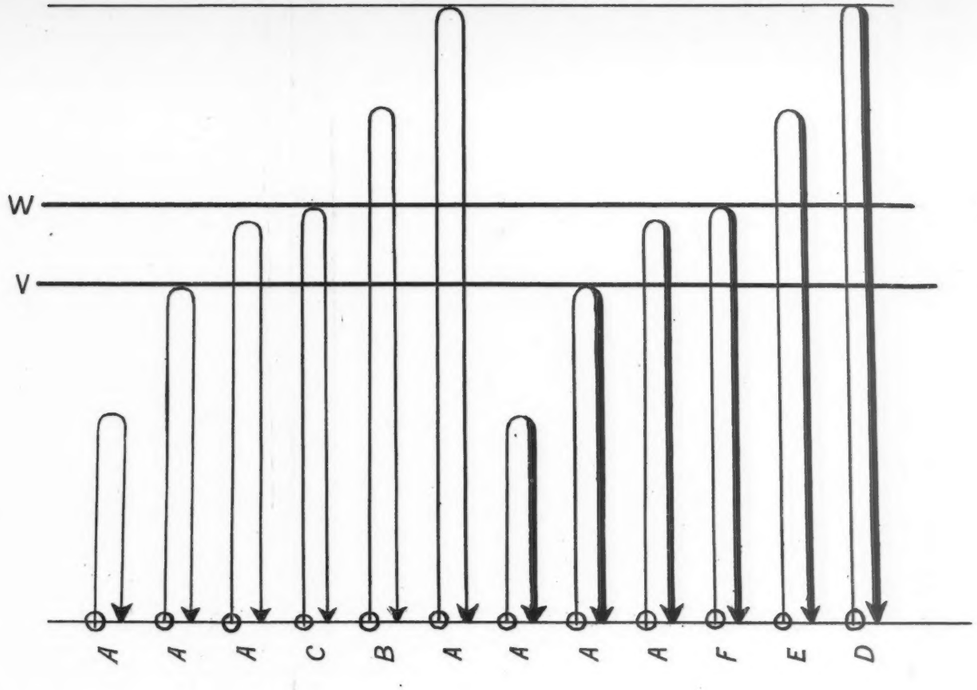
All who have studied the subject of the strength of guns will agree that the principle of "initial tensions" must be utilized to the greatest possible extent in any system of gun construction. That stronger guns of the same weight and caliber can be made by its aid than by any other process which does not use it in some form.

This important principle is best illustrated in the Rodman process of casting, if it could be carried out to the theoretical limit, which is to produce an initial compression of the bore equal to the elastic limit of the metal for compression, the outside layers of the gun being in a state of initial tension. Now the question arises: What effect has the process of annealing on the initial tensions of a hollow cast steel gun? The popular idea is that the annealing of steel is resorted to primarily to get rid of the tensions which may exist throughout the mass, and if that is the effect of annealing, what becomes of the increased strength of the gun which is obtained by putting the bore in initial compression due to external tensions. It is an absolute necessity for the steel cast gun to retain the initial tensions produced by hollow castings, because if the gun by annealing is allowed to return to a normal state, a homogeneous tube of excellent steel, it can be shown without a doubt that it cannot compete in elastic strength with a steel built-up gun of the same weight and caliber in which equally good steel is used. Take as an illustration a 6-inch gun. At the powder chamber the internal and external radii are 3.5 inches and 10.25 inches respectively. If the steel has an elastic limit of 45 000 pounds per square inch, the safe powder pressure which will not deform the bore is found by Clavarino's formulas to be 13 tons. But we already use a powder pressure of 15 tons per square inch, and hope soon to be able to use 20 tons when wire-wound or ribbon-wound guns can be successfully built. The built-up steel gun of the same dimensions and weight, built of equal quality of steel shows a safe powder pressure of 20 tons per square inch. So the built-up gun is the superior as to strength over the steel cast homogeneous gun in the ratio of 20 to 13. If on the other hand the initial tensions can be retained to the extent of compressing the bore to its elastic limit, then the steel cast gun could withstand a safe pressure of not less than 25 tons per square

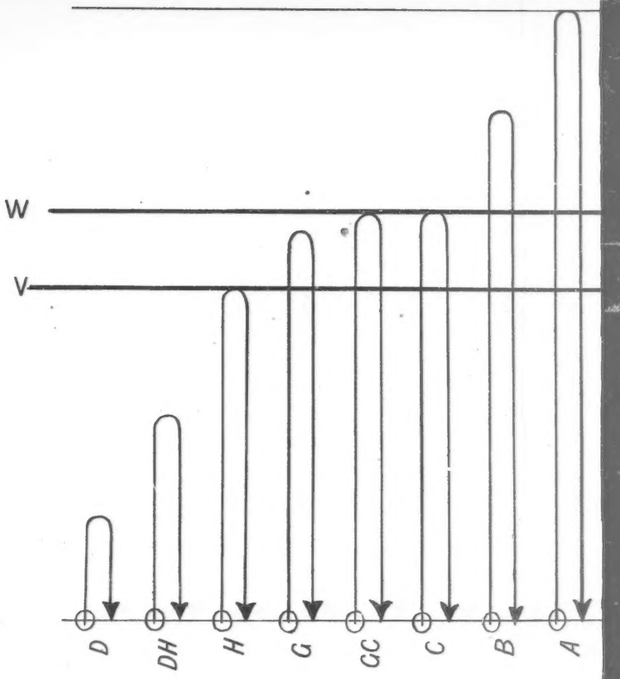
GROUP 1.



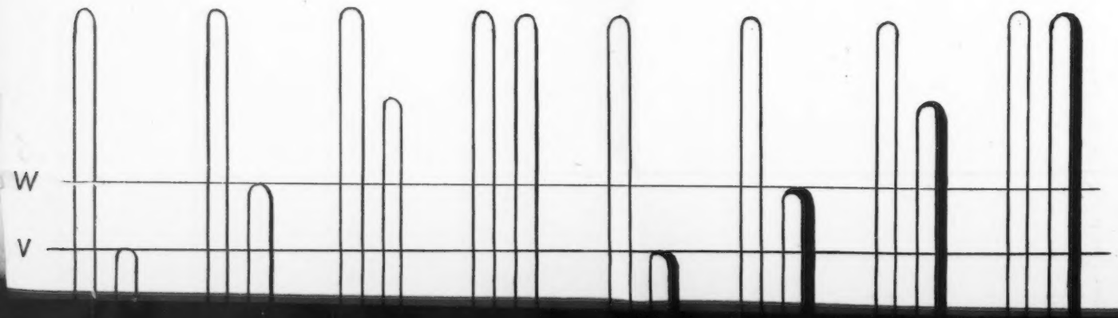
GROUP 2.



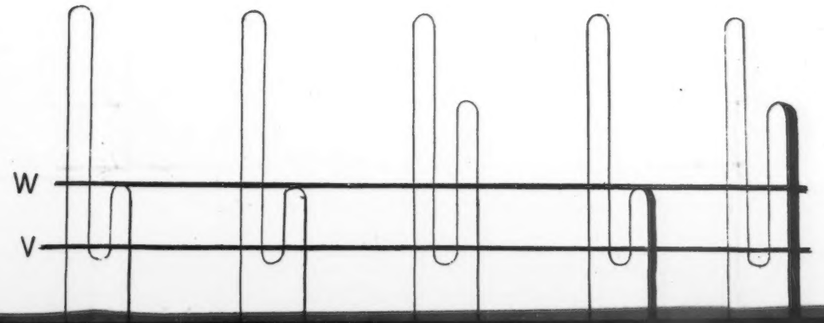
GRO



GROUP 6.



GROUP 7.



GROUP

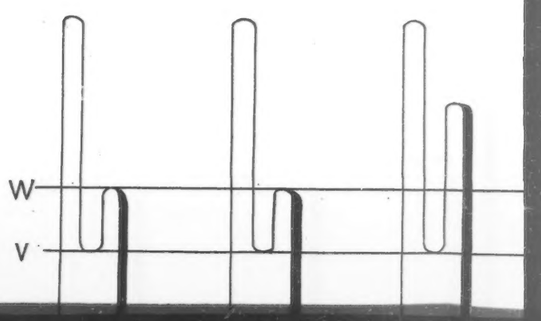
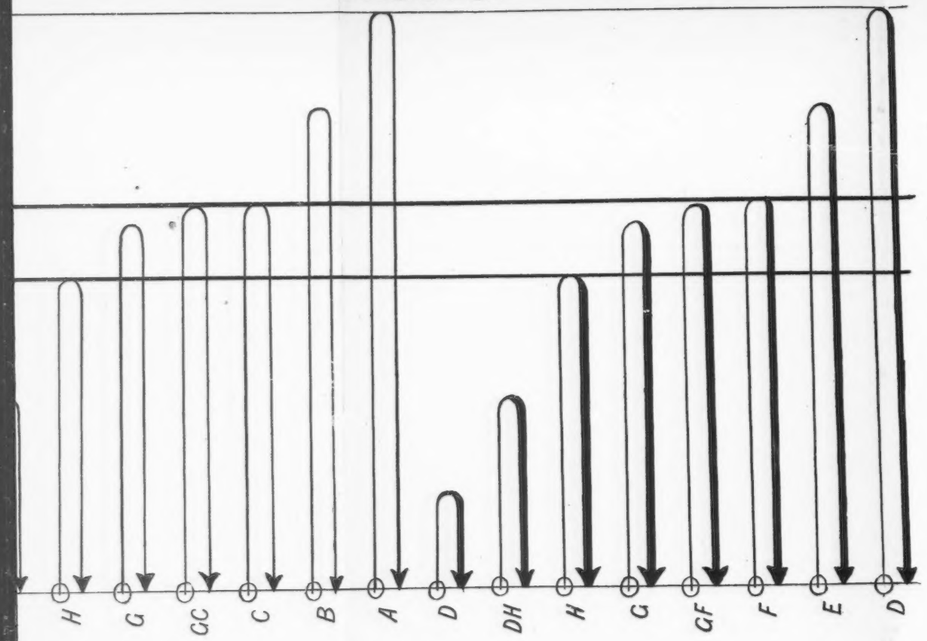
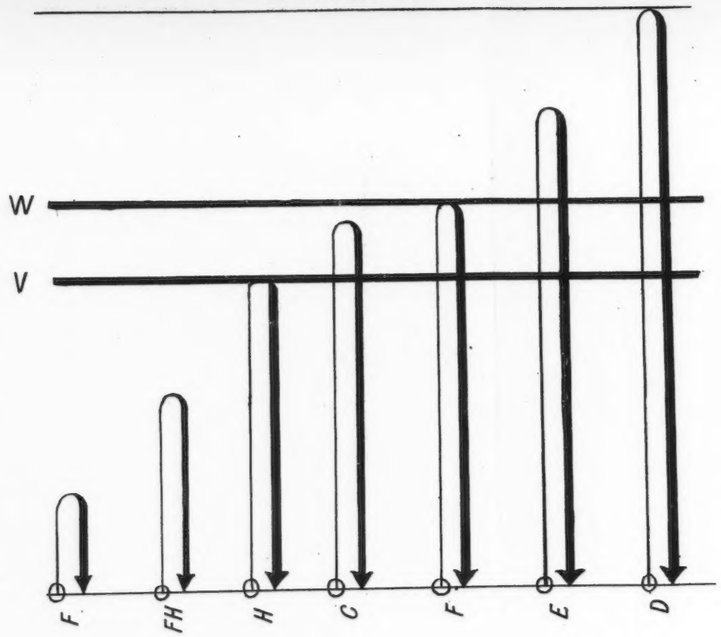


PLATE XXIII
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI NO 361.
COFFIN
ON STEEL

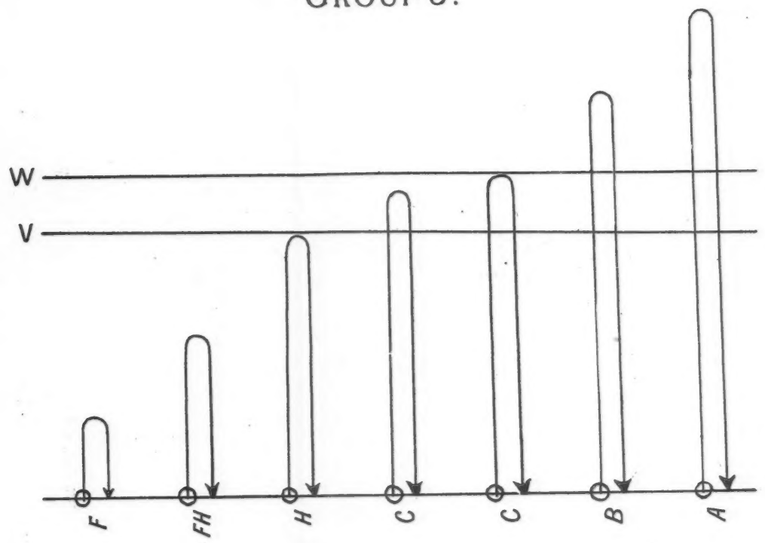
GROUP 3.



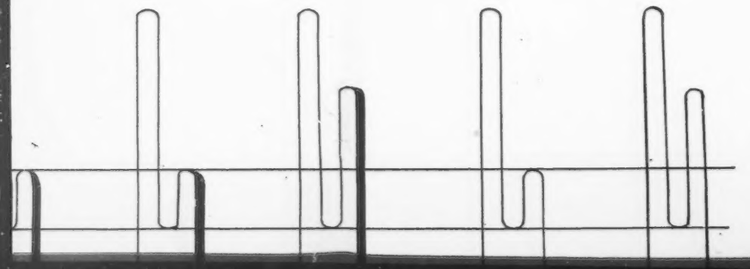
GROUP 4.



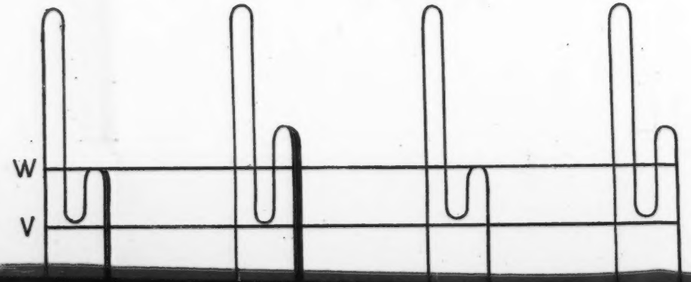
GROUP 5.



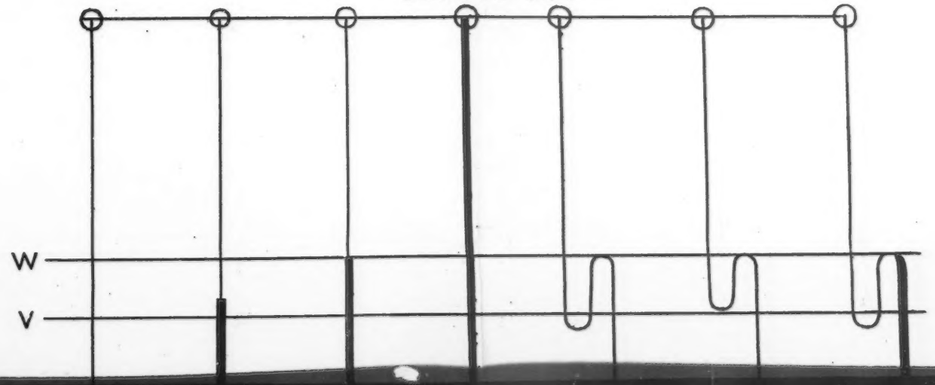
GROUP 8.

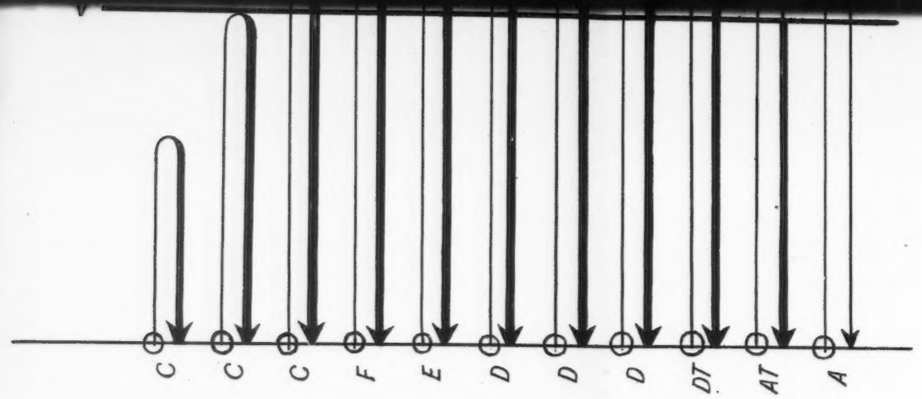


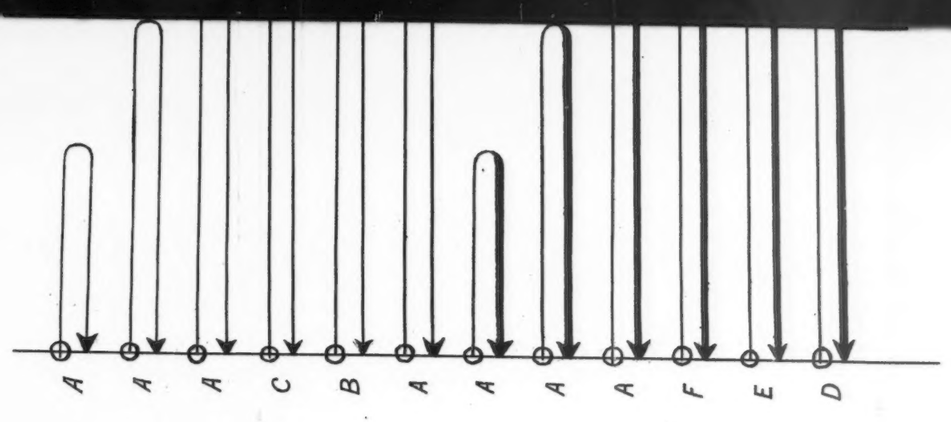
GROUP 9.

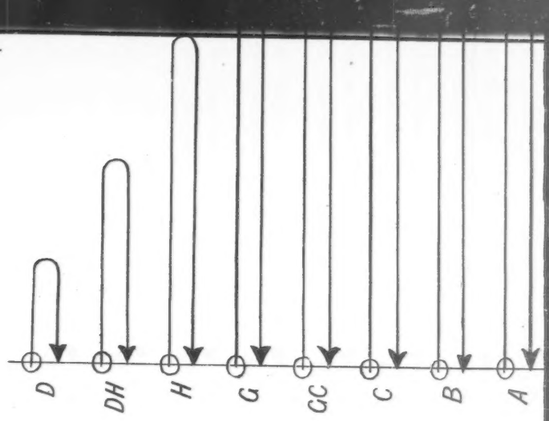


GROUP 10.





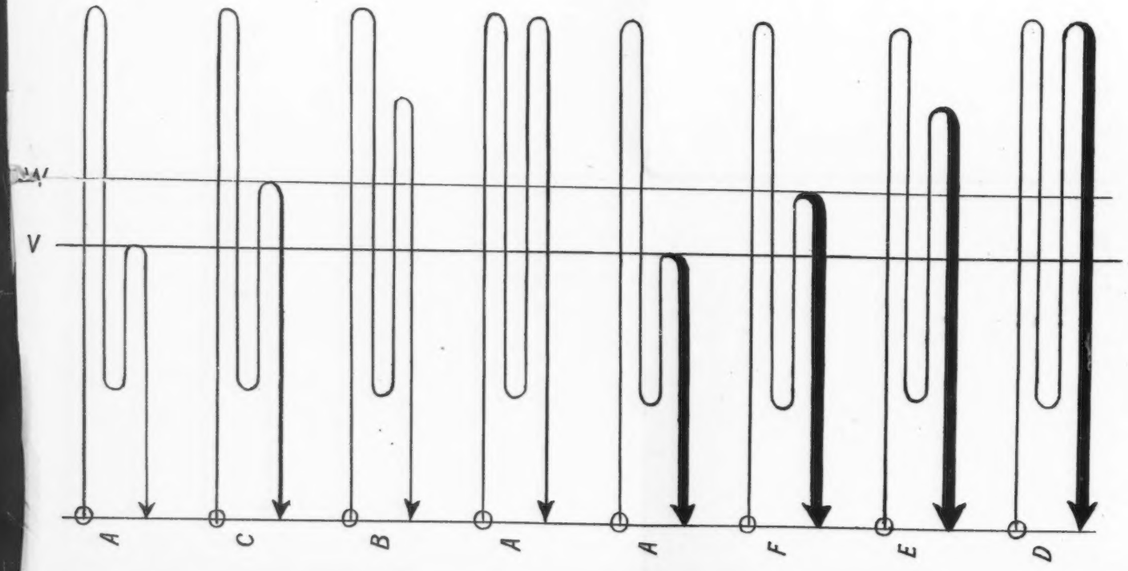


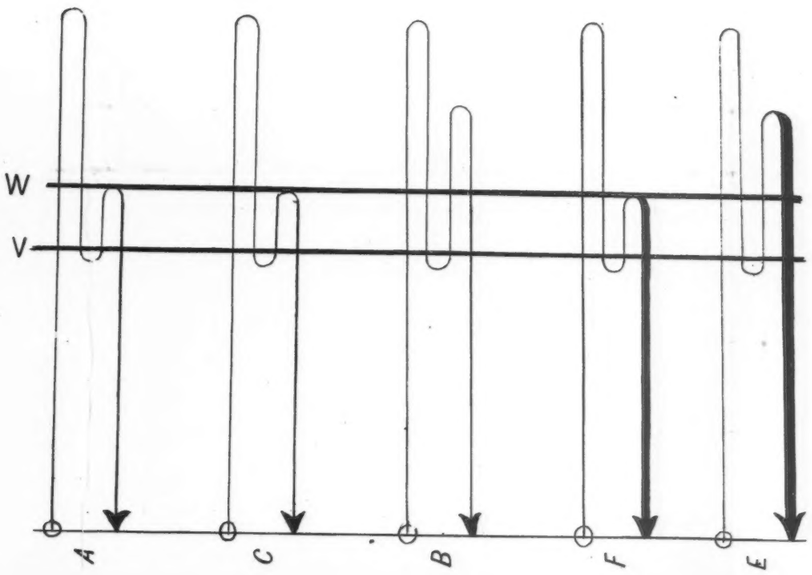


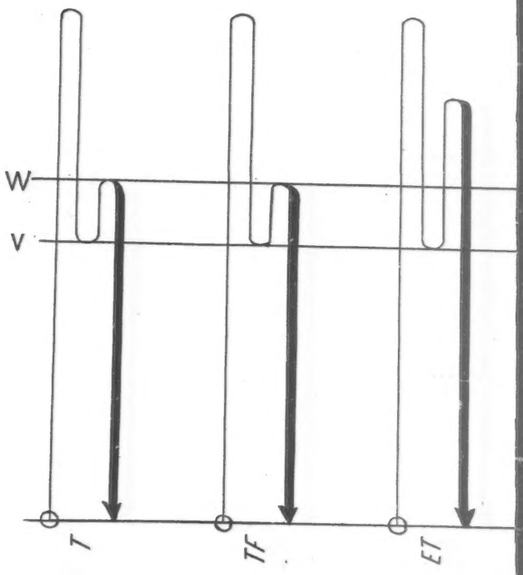
GROUP 6.

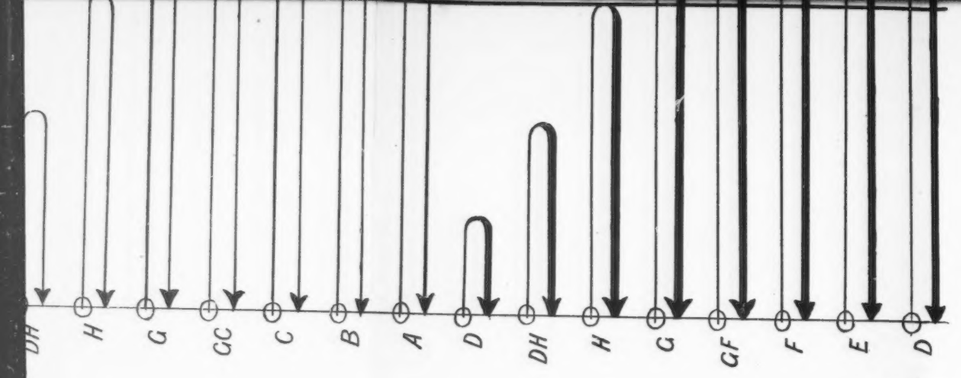
GROUP 7.

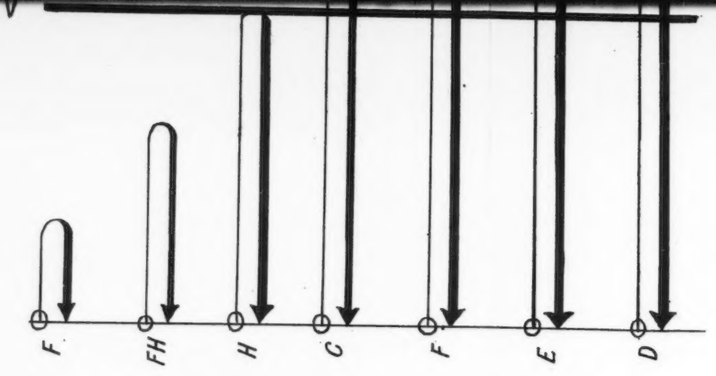
GROUP

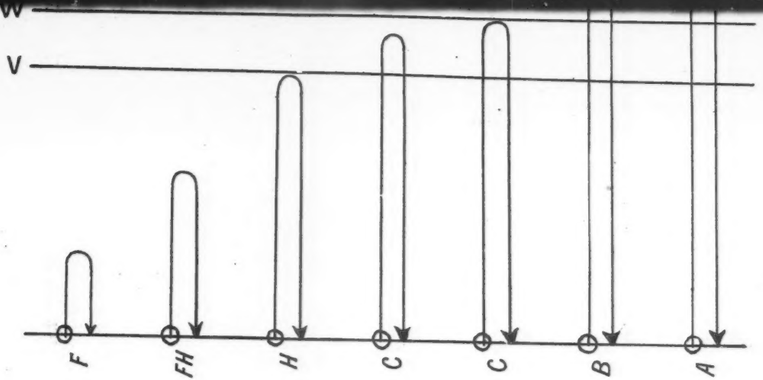




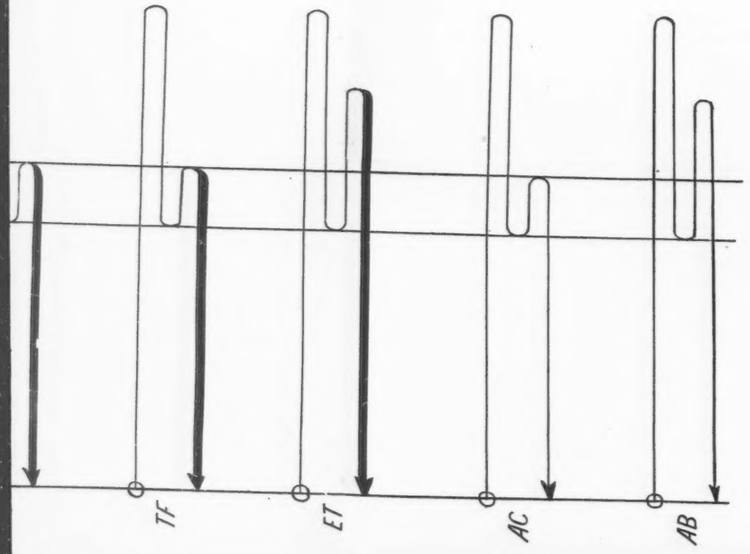




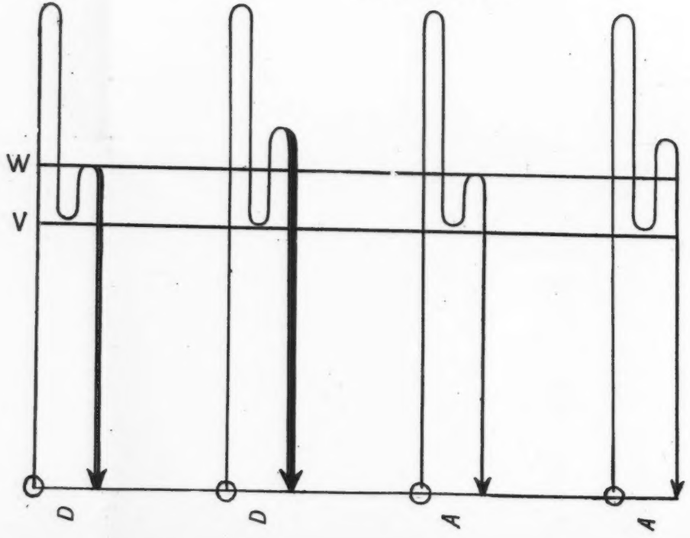




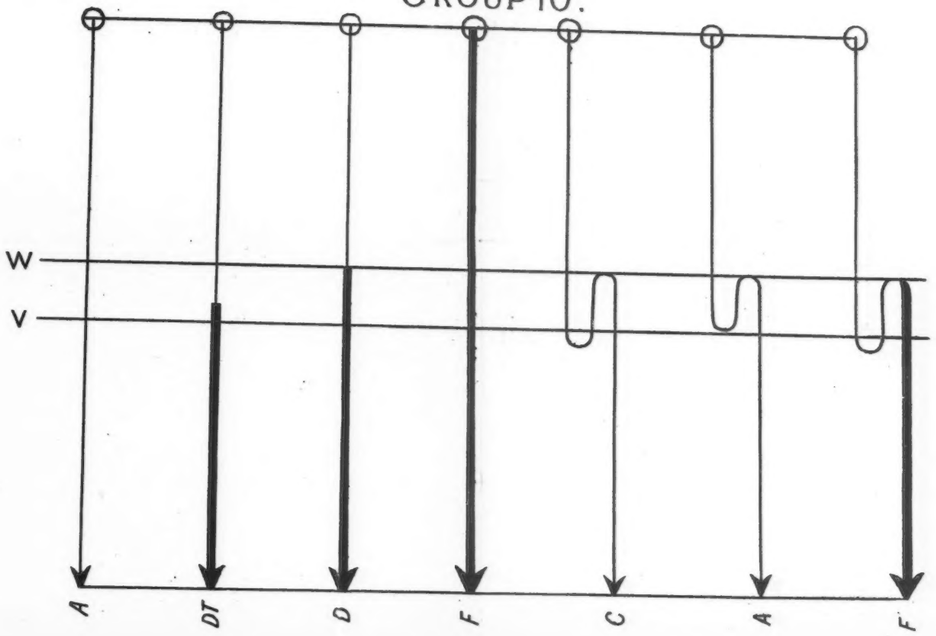
GROUP 8.



GROUP 9.



GROUP 10.



1

2

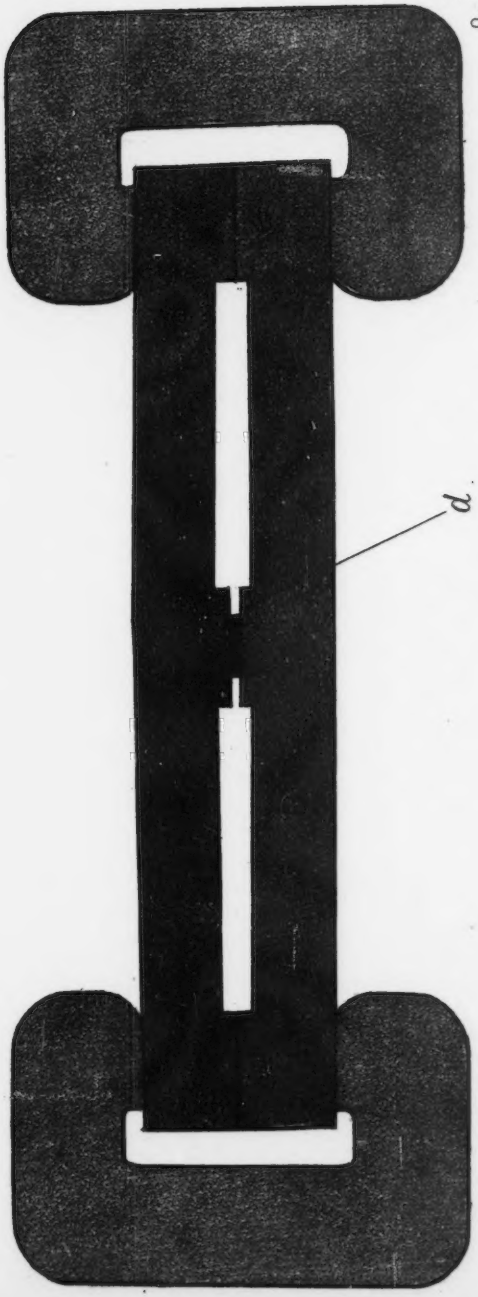
3

4

PLATE XXII
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 361.
COFFIN
ON STEEL

5 6 7 8 9 10

PLATE XXIV
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI Nº 361.
COFFIN
ON STEEL



inch. It would seem then that the annealing should not be allowed to disturb the initial tensions produced by casting.

It would perhaps aid in convincing skeptical minds if the lecturer would indicate how the elastic strength of the steel cast gun would be computed. The strength of guns should be a matter of computation just as surely and accurately as in the case of any other mechanical structure.

Mr. Metcalf is a little severe on those officers who view with some concern the efforts which have been made from time to time to change the plan of gun construction which has thus far been followed, but I venture to think that if for years past he had been obliged to witness, in common with naval officers, the smile of derision and contempt with which our miserable show of force is greeted abroad whenever our ships enter the ports of civilized powers, he would perhaps appreciate the feelings of those who, having seen the work of gun construction so happily begun, cannot help but show uneasiness at any influence being brought to bear to delay that work. I do not believe that officers of the navy, as a body, are unwilling that the steel cast gun should be tried; they are not willing, however, that all work should cease until it is tried. It may or may not prove a success; but, if successful, does any one doubt its adoption?

I beg to thank the Society for the honor of being allowed to discuss this important subject. It is a matter for congratulation to naval officers that the support of this able and scientific body of men is thrown in favor of steel guns in some form, because it has been a fight to get guns built of steel.

Mr. WILLIAM METCALF.—In my own mind I thought always of casting the gun hollow by the Rodman method, but the difficulties of managing the core would be so great, that I believe Mr. S. T. Wellman's plan would be wiser; *i. e.*, to cast the block solid, bore out the center, and then proceed with the treatment. This is no modification of the principle, but it simplifies greatly the whole operation, and removes from my mind the only doubt I ever had about the feasibility of the plan.

What I have tried to say and what I adhere to, is this: I believe a large mass of steel can be treated by heat more surely, more cheaply, and better, than by any known mode of forging; and that by cooling on the Rodman principle a gun can be made to have with absolute certainty, the tension in exactly the direction and to the degree that is most desirable.

In answer to Mr. Ingersoll's question about computations, I beg to be excused. Our officers have proven themselves masters of the art of calculating, while I have to admit that during twenty-nine years of shop-work I have had little time to think of mathematics, and that is the reason why I have confined myself in this paper rigidly to physics.

Lieut. AUSTIN M. KNIGHT, U. S. N.—As I understand Mr. Metcalf, he has no dispute with ordnance officers as to the qualities which a gun should possess. The high tensile and elastic strength and the power to resist erosion which they demand, he demands with equal emphasis. But he believes that these qualities can be obtained with greater certainty and at a less cost in a cast than in a built-up gun. In this most officers of the army and navy differ from him; but their difference is, I believe, a matter of honest conviction, and not, as he implies, of narrowness and exclusiveness.

The feeling of the navy, at least towards such civilians as Mr. Metcalf and Mr. Dorsey, is, so far as I have been able to sound it, one of extreme cordiality. Mr. Metcalf questions this I suppose, because he has been for some years preaching the virtues of a cast gun without securing a test of his system. It would seem that the responsibility for this neglect lies rather with Congress than with the army and navy. So far as the navy is concerned, the money that has been appropriated for guns within the last five years has been appropriated for a certain definite purpose—the production of a specified number of guns of a specified caliber.

The officers upon whom has devolved the carrying out of this purpose have had before them then the problem: Given a sum of money, how shall that sum be invested to produce a certain number of guns of given weight and power, and of the greatest attainable degree of reliability? They have had no lack of systems of construction from which to choose.

Mr. Norman Wiard and some others have told them that the gun for their purpose should be made on Mr. Wiard's principle, and of cast-iron. Ten such guns were tested in 1873. Of these four burst, and one was complely ruined at the first fire.

Mr. Haskell has advocated a multicharge gun, also of cast-iron. One such gun has been tested, it burst at the fifty-third round.

Mr. Metcalf, supported by Captain Michaelis—a high authority in all matters of ordnance and military engineering—has urged the adoption of a steel gun cast on the Rodman principle. No such gun has ever yet been made.

Most officers of the army and navy have favored a built-up gun. Some fifty thousand such guns are in service abroad. In fourteen years about ten of them have burst. Others have failed, but not in the way to endanger life.

Having before them the above systems and their records, or absence of records, the navy might have decided to test them all; supposing that the Acts of Congress involved could have been so construed as t admit of such an expenditure of the money appropriated. But nov comes in the question: What constitutes a test? Mr. Metcalf and his supporters ask that a few guns on their system be made and tried. But

from their own point of view this could prove but little as to the merits of the system. Fifteen built-up guns have already been made and severely tested in this country without a single failure, and still Mr. Metcalf insists that these guns are unreliable. If the success of fifteen built-up guns proves nothing, would the success of three cast-steel guns prove more? If not, where is the test to stop? When will it be admitted that gun manufacture in the United States (under whatever system) has passed beyond the experimental stage, and when may the army and navy proceed with the armament of our ships and forts without being continually called upon to stop and wait until it can be learned whether some other system is not better than the one they are using?

Mr. Metcalf is right in holding that not the success of fifteen, nor the success of fifty guns, can prove the absolute reliability of a system. Nor should the failure of fifty guns in fifty thousand be held to prove its absolute unreliability. So long as gunpowder remains the mysterious agent that it is at present when confined and burnt in a gun; so long as the effects of variations of temperature and moisture accompanying its manufacture and its explosion are as little known as at present; so long as the nature of the vibrations which it communicates to the gun remains uninvestigated, and the tension which it exerts upon the walls of the gun remains but imperfectly determined; so long as we are ignorant of the changes effected in steel by variations of climate, by repeated vibrations, or by long periods of rest from all vibrations, so long will there be some danger of failure with any system of construction.

The tests then of the cast-steel and other systems proposed, could not, even from Mr. Metcalf's point of view, have stopped with any moderate number of guns. They must have been carried so far as to involve vast expense and long delay in the beginning of a work which Congress and the country, as well as the army and navy, felt to have been already too long delayed.

The alternative was to adopt that system which had the weight of evidence in its favor, and, having adopted it, to push forward in the work of national defense along the road thus entered upon without stopping at every stage to wonder if some other road would not have been better. This was the alternative accepted.

The built-up gun was adopted, but with many changes from European models. European nations had learned much in twenty years of experience with such guns, and we were able to reap the advantage of their experience. The latest designs of all governments and manufacturers were studied, and an effort made to take the good and reject the bad features of all.

As the enemies of the new navy never weary of asserting that the models of the English government were blindly followed, it may be

worth while to remark in passing that the design adopted differed rather more widely from the English system than from any other in existence in 1881, the year when the present Naval Bureau of Ordnance design was made.

At that time, although an English Ordnance Committee had experimented with breech-loading guns, no such gun had been adopted for service. The English guns of that period were muzzle-loading, and were built up of steel and wrought-iron. It was nearly two years after the American naval design of a breech-loading all steel gun was definitely adopted that the English Government accepted these two most important features.

All the same, certain of the followers of Mr. Metcalf and Captain Michaelis, not those gentlemen themselves, will continue to the end of the chapter to assert that Woolwich designs have been blindly followed, and that we have therefore to anticipate only a repetition of English experience.

I have said that most of the officers of the army and navy differ from Mr. Metcalf's theories. Their grounds of difference I have not attempted to explain, because they have been already stated. But though, in common with other officers, I dissent from Mr. Metcalf's views, I am not opposed to a test of his system. I should be glad to see it tested. But the test should be thorough; it should be applied to many guns, and to guns of various calibers. It will require large sums of money, which should be specially appropriated for the work, and much time, which in the present state of our national defenses we cannot afford to lose.

I protest against the demand that at this time we should stop work on guns which we know to be as good as any others in the world, to experiment for a few years with guns which Mr. Metcalf hopes may be better than any others. When the country shall have been placed in a state of tolerable defense with guns that we know about, then it may be good policy to devote our money and our time to experiments upon promising theories. If it shall then appear that we have expended more money than was necessary, we shall be only in the position of a man who has insured his property, and whose house has not, after all, burned down.

Mr. Metcalf would reject, as only less barbarous than a hammer, the hydraulic forging press; yet he attaches great value to the forging effect of statical pressure in the molten metal itself. To me there seems little difference in the effect between this pressure and that of a hydraulic press, except that the latter would be felt equally throughout the mass of the metal, while the former is very great at the bottom but decreases rapidly toward the top. With regard to the question of cost, Mr. Metcalf gives no figures that can apply to his gun. True, he tells us that splendid castings may be had for six cents a pound, but he surely does not mean that a gun can be cast and finished on his plan for any such

price as that. I would remark also that what is a "splendid" casting for ordinary commercial purposes might be a very poor casting for a modern high-power rifled gun. What is needed here is not a splendid casting, but a perfect one, if any.

The position of ordnance officers with regard to the question of expense, is simply that the cost of a system of gun construction must always be held as secondary to its efficiency. There is only one good gun, and that is the best gun. A system therefore which appeals for their support primarily because it is cheap, stands at a disadvantage as compared with one whose first claim is proved efficiency.

Just at this time, moreover, officers hold that in the question of our national defense, time should also be considered before cost; and in this I believe they have with them a large majority of the taxpayers of the country.

In conclusion I desire to say that it seems to me a matter for earnest congratulation on the part of the army and navy that the civil engineers of the country should have come to take the active interest in military affairs of which this discussion is an evidence. From the interest and co-operation of a profession so closely allied to their own, officers may expect to derive assistance and instruction of the greatest value. If the instruction comes in the form of criticism as honest and as friendly as that of Mr. Dorsey and Mr. Metcalf, it will not fail I think of frank and full consideration.

I beg to express my thanks to the Society for the privilege of taking part in this important discussion.

Mr. WILLIAM METCALF.—Mr. Knight says I charge officers with "narrowness and exclusiveness." I certainly had no such intention.

My whole object has been to call attention to the possibilities of the casting system for the purpose of producing, economically, very large guns, say 100 or 150-ton guns, and to urge that Congress should direct that the plan be tried. I fail to see how this would interfere necessarily with the work of the navy and army as at present conducted.

Their guns are good, undoubtedly; let them go on making them as fast as they can, and their operations need not be retarded one day by other experiments. They allow now thirty months for preparation for what they want. The Otis Iron and Steel Company, of Cleveland, can make an 80-ton casting with their existing plant, and if they were authorized to cast a gun, or guns, they could be made, finished and tested inside of one year.

Is it not true that if the Ordnance Departments would recommend such a trial, even faintly, Congress would grant the necessary means?

Mr. WILLIAM SELLERS, M. Am. Soc. C. E.—In this discussion I propose to confine my remarks to the question of guns, for the reason that I find no other point in Mr. Metcalf's very able paper upon which to

hinge a discussion; and even as to this, I am not prepared to say that the American gun of the future will not be a solid mass of cast steel, but only that I doubt it; because, although this may prove to be the cheapest way to make great guns in the future, it does not appear to be so now, and with the improvements in progress it is less likely to be so hereafter.

If we could assume that every great casting for a gun would prove perfect, one very important element of cost would be eliminated; but if the present high standard of quality is to be maintained, it may be safely affirmed that a notable quantity of such castings will be condemned, and then what shall we do with them? We cannot permit such vast masses of material to cumber the ground, and it will cost far more than the material is worth to cut it into pieces small enough to remelt; and yet this is all that we can do, and the cost of doing it must be added to the cost of the good castings.

The casting of an ingot heavy enough to make the tube of a sixteen-inch gun, boring it and forging it to the proper length, would be accompanied with far less risk than to cast the same weight in one piece of that length, with a core throughout cooled on the Rodman principle; and I believe that the cost need not be greater, while the quality of the material would be far more uniform in the forged piece, not solely because it was forged, but because the quality of the short ingot would be more uniform than that of the long casting.

That the built-up gun is unmechanical, or that the quality of the material in such guns cannot be had in as perfect condition as in the solid casting, I do not believe. That the material is not in its best condition in built-up guns as now constructed is, I believe, true; but the principles upon which the several parts of such a gun should be manufactured have long since been published to the world by Rodman, and the application is simply a question of time. That this principle has not been applied to the built-up gun demonstrates that man is rather imitative than original in his habits. So long as it is a gun that is to be made of cast-iron, the Rodman principle is applied just as Rodman applied it; but when it is only a part of a gun, and that part not of cast-iron, it would seem that the same principle had no place, and yet that principle is far more easy of application in the built-up gun than in that to which its discoverer applied it.

That principle is, prevent radiation from the exterior and cool from the interior.

The comparatively short ingot from which the tube is made can be cooled from the interior far more easily than the long cast gun, and in every step thereafter, in the construction of the built-up gun, the same principle can be applied with greater facility than in its first application to the casting. The oil tempering, or rather the tempering, for oil is not necessarily the medium, may be done on the same principle, and

when the several parts are finished they may be assembled in observance of the rule that the exterior must be the last portion to cool; and whenever this is done, the shot from the American gun will attain a higher velocity, while the endurance of the gun itself will be superior to any heretofore constructed to any that will hereafter be made in one solid piece.

Mr. CHARLES A. MARSHALL, M. Am. Soc. C. E.—In discussing this paper I shall mainly devote the limited time at my disposal to criticising the argument and conclusions of the author as regards heavy guns.

The subject of ordnance is a broad field for discussion and equally as broad for investigation. The newspapers frequently have a fling at ordnance bureaus and the like, and many men with theories have succeeded in getting them aired in print, all tending to discredit the scientific and professional standing of the officers who are responsible for getting the best return in guns and material for the people's money devoted to that purpose. The particular proposed kind of gun advanced by Mr. Metcalf has probably more and more intelligent civilian backing than any other. It certainly deserves careful consideration; but are we to suppose that it has not been studied by ordnance experts? I am not informed on that point, but refer to it in this way because I have tried to look at the subject so as to form an impartial judgment based on facts, as should those on whom rests the responsibility of spending money for none but good guns, and of doing effective work with them when the emergency for their use shall arise. Responsible persons cannot afford to make blunders; it is not only fair but logical to give them the benefit of the doubt till they are proved wrong; and in this instance I believe and hope to show that the advantages are greatly in favor of the hooped as against the cast guns.

The conditions to be met by a gun to-day are different from those required in the day of Rodman cast-iron guns, referred to by the author as being "to batter down an earth-work or to sink a ship." The earth-work of to-day has its guns of position protected by heavy armor of steel or of chilled iron; the ship has heavier and more impenetrable armor. These facts imperatively demand that the piercing power of our weapons be increased; that is to say, the foot tons of work that the gun has to do in order to be effective must be given in larger doses to the separate projectiles. To reach this result by larger bores, and heavier projectiles thrown with diminished powder pressure as compared with the best guns made to-day, I presume all will admit would be a step backward.

Rifled guns with elongated projectiles capable of using high powder pressures are acording to all our lights the guns we want. Since it was pretty definitely ascertained by Whitworth that there is a certain best ratio of length of projectile to diameter, the conclusion is irresistible

that improvements in guns to-day should be in the direction of adapting them to higher powder pressures. Increasing the length of bore with the use of slow burning powder is, of course, another way to gain velocity and increase the energy of a shot, but this has been done; the disadvantage of unwieldiness presently puts a stop to improvement in this direction. Now I submit that the author has not even given a glimmer of hope that the cast-steel gun will bear higher powder pressures than the built-up gun. His proposition is mainly to give as good a gun at less cost. The question is whether this is possible.

It is not claimed that cast metal can be so treated as to have a higher limit of elasticity than steel that has been forged, only that it can be brought to equality. For the argument I shall take this for granted (though I do not believe it, never having seen an example to prove it, and having seen plenty proving the contrary; and I am surprised that one so conversant with his subject as Mr. Metcalf, should ignore in his argument the beneficial effects of work on steel), and I shall take for granted at present also that the reliability of the two metals will be equal, but wish distinctly to note the reservation that the conditions of casting and of treatment by heat must be equal or equally favorable.

As to conditions of casting, the built-up gun is made of moderate sized ingots which cool quickly; thus the tendency to separate out the carbon and other metalloids is minimized. The only limit to the rapidity of cooling is that it shall not cause cracks; this is entirely controlable. Piping of ingots as a source of injury to the steel is eliminated by boring out the central portion of all ingots whether piped or not. This also improves our material if segregation of metalloids has taken place, since the center is the most injuriously affected by that defect. Against these important advantages as affecting integrity and uniformity of material, we have the sole advantage in favor of hollow castings that the metal will be prevented from cracking by being cooled from the center out. It is assumed that the top part of the casting will be thrown away in each case. Disadvantages of casting the gun as a whole on the Rodman system are:

First.—Enormously greater weight and size of casting; largely increasing risk of making. Cast-iron is easier to handle than steel, yet the South Boston Foundry lost three castings before they succeeded with one.

Second.—Very much slower cooling, which is the condition favoring ununiformity by segregation. The Rodman process doubtless seems to some minds a quick process of cooling, which is so, considering the size of the castings. Yet General Rodman states (Senate Rep. Com., 266, XL Congress, 3d Session, p. 80) that the time of cooling a 15-inch hollow-cast gun was about six days, and that the outside temperature did not fall below 500 or 600 degrees temperature for about two days.

Third.—As another consequence of the slow cooling, large, weak crystals.

Fourth.—As another consequence of the slow cooling, a very low elastic limit to the outer metal certainly, and for the inner metal a higher value, but for the same kind of steel not anything to approach the treated gun tube quoted by Mr. Metcalf from Holley. The elastic limit to the outer metal would be not over 29 000 pounds per square inch for metal having same carbon contents, as gun-hoop steel, which, it may be borne in mind, is the hardest steel used in the guns to day—such metal as Cambria Iron-works are producing in hoops with an elastic limit of 55 000 pounds minimum by specification.

The following three tests from the same ingot exhibit this:

Specimens .564 inch.; diameter, 6 inches gauged length.

Test—How taken.	Elastic limit.	Ultimate strength.	Elongation.	Reduction of area.
	Lbs. per sq. in.	Lbs. per sq. in.	Per cent.	Per cent.
From casting, natural	29 000	82 400	4.9	6.6
From 3 inch square, cut out of ingot and annealed; cooled in open air	46 000	100 560	12.1	15.0
From forged and treated gun-hoop	58 000	107 240	13.1	23.1

The following examples of elastic limits from various castings, all having in the neighborhood of 0.50 per cent. carbon, go to show what we may expect in large castings annealed. The above example of piece 3 inches square, it is seen received a higher elastic limit than can be obtained by annealing the larger masses.

	Elastic Limit.
48-inch roll casting, annealed, two tests	34 000 36 000
Blooming-mill pinion, annealed, two tests	34 000 33 000
Blooming-mill pinion, annealed, four tests, first two at top, second two at bottom as it lay in pit	36 000 34 000 38 000 39 000

I will now refer to the army 8-inch hooped steel rifle recently built and successfully tested. The data for this gun are given in Report of Chief of Ordnance U. S. A., 1885.

Refer to section at powder chamber. Dimensions are inches; pressures, pounds per square inch.

Internal diameter, 9.5.

Thickness of tube, 2.25; of jacket, 4.0; of first hoop, 2.15; of outer (second) hoop, 2.6.

External diameter, 31.5.

Limiting value of metal in tension at interior of outer hoop, where maximum, 50 000.

Maximum tension under fire at interior of outer hoop not given, but

if the internal pressure were 55.500, then the maximum tension would be equal to elastic limit = 50 000.

Maximum tension, system at rest (.001163 × E) = 34 900.

Initial compression of internal fiber of tube (.0016 × E) = 48,000, approximately equal to elastic limit.

Powder pressure, since used on trial = 36 000.

Next I shall inquire just what must be the condition of initial strain in a homogeneous cylinder to enable the condition of the above gun to be fulfilled, viz.: no part to be stressed beyond the elastic limit by an interior pressure of 55 000 pounds per square inch. At the outset we are confronted with an utterly unknown factor—the law of variation or distribution of the stresses caused by any given mode or rate of cooling. It is, however, near enough for present purposes to assume that the stress varies regularly from a certain compression inside to an equal tension outside, since, as we shall see, what might be gained in one place would have to be practically lost in another. It would be of interest and value if some advocate of cast-steel guns could by scientific experiments, which need not cost more than a few hundred dollars, demonstrate the law upon comparatively short cylinders. Of course the difficulty of so treating longer cylinders as to arrive at definite results would remain, but such experimental data would help to determine the question of ultimate possibility of success which is yet unproven.

To proceed with this inquiry. The elastic limit of the interior metal must be as great in the cast gun as in the hooped one, since here will be found the severest stresses, both radial and tangential, 50 000 is this value.

Assuming that the gun is not treated after casting, we must take 29 000 as elastic limit of external metal as already given. By subsequent treatment, as to be later referred to, it seems just possible that an external elastic limit of 40 000 might be reached, hence I have included this case in the table and diagrams.*

---

* The formulas are the following (Longridge, "Treatise on Application of Wire to the Construction of Ordnance," pp, 35, 36), external pressure being taken = O.

$$t_i = f_o \frac{m^2 + 1}{m^2 - 1} \quad . \quad t_e = f_o \frac{2}{m^2 - 1}$$

$$t_y = \frac{t_i (R^2 - y^2) p^2 + t_e (y^2 - p^2) R^2}{y^2 (R^2 - p^2}$$

in which

f_o = internal pressure
t_i = " tension
t_e = external "
t_y = tension at y
R = external radius
p = internal "
$m = \frac{R}{p}$.

Exactness was not aimed at in the calculations.

TABLE No. 1.

$m = \frac{\text{External diameter}}{\text{Internal diameter}}$. All pressures in pounds per square inch.

Assumed elastic limits.	m	Permissible initial tension.	Internal tension under 55 000 pressure.	External tension under 55 000 pressure.
Internal = 50 000	3	15 250	53 500	29 000
External = 29 000	3½	15 880 to 18 120	47 760 to 50 000	26 760 to 29 000
	4	12 330 to 21 670	40 660 to 50 000	19 660 to 29 000
	5	9 580 to 24 420	35 220 to 50 000	14 160 to 29 000
Internal = 50 000	3	18 750 to 26 250	42 500 to 50 000	32 500 to 40 000
External = 40 000	3½	15 880 to 29 120	36 760 to 50 000	26 760 to 40 000
	4	12 330 to 32 670	29 660 to 50 000	19 660 to 40 000
	5	9 580 to 35 420	24 160 to 50 000	14 160 to 40 000

The smaller limit of permissible tension in table is obtained by subtracting 50 000 from the internal tension in an initially unstrained cylinder that would be caused by an internal pressure of 55 000 pounds per square inch. The greater limit is equal to the external elastic limit less the external tension due to same pressure. In the diagrams, Plates XXV and XXVI, tension is measured up from *O Y*, compression down; *P* is the internal radius and *O Y* the external.

The important thing to observe is the narrow range of permissible initial tension. In the case of diameter equal to the army hooped gun ($m = 3\frac{1}{2}$) the range is only 2 240 pounds per square inch, with external elastic limit = 29 000, and 13,240 if that limit be 40 000. Plainly the gun cast without subsequent treatment is condemned to increase in diameter and weight. Increase diameter to $m = 5$, giving outside diameter = 47.5 inches, or as great as a 12-inch rifle if built up, and the foundryman still must hit within 14 840 ÷ 2 = 7 420 pounds per square inch of a certain definite stress. But it is not worth while to pursue the subject of casting without subsequent treatment any further. My own opinion is, considering the difficulty of making ordinary castings, even of iron, but especially of steel, always sound, that the cooling from center and outside will have done its full duty if it produces castings without cracks; I should even then expect frequent cases of hidden cavities. That is to say, if we can keep within a range of double the elastic limit we shall do very well. Note that it is external surface we have to deal with, since the internal surface is hard and unyielding practically at the time the initial strains are induced. Expansion of iron or steel under heat is about .0012 of length for 180 degrees Fahr. Figures would indicate that working within a difference of temperatures of 290 degrees Fahr. saves cracking the casting whose external limit of elasticity is 29 000 to per square inch.

Now to consider the possibility of treating a hollow cast gun so as to equal the built gun in efficiency. The range of initial tension when $m = 3\frac{1}{2}$ has already been referred to; it is 13 240 pounds per square inch = 66 degrees Fahr. of temperature. Aiming at the middle of range, I would inquire: Has anybody the means of registering temperatures at an orange or high red heat so closely as 33 degrees Fahr. with any certainty? If so, and if means could be provided for testing the effect each time, since different melts of steel vary in the temperature at which they are affected similarly, there would be possibilities ahead in this line.

Recurring to the tests of castings given on page 345, 39 000 pounds per square inch is the highest elastic limit. Specimen was taken from the neck of pinion, hence repesents a smaller casting than the guns we are considering. I would call attention to the vital fact, not mentioned in Mr. Metcalf's paper, that size very greatly affects the results of treatment by heat. One cause of this is however mentioned in the paper,

viz., that slow cooling favors the formation of large crystals, which are a source of weakness and more especially of brittleness and untrustworthiness. There is another cause, which is that slow cooling permits a change in the condition of carbon; this takes effect principally at a high temperature or, according to Brinell (*Eisen und Stahl*, November, 1885; also "Notes on the Construction of Ordnance, No. 37," Ordnance Department, U. S. A.), almost altogether between certain two temperatures which for the same steel are quite well defined. The change is from "hardening" or "combined" carbon to "non-hardening" or "graphitic," as it has been called, though it is pretty well ascertained that the dark looking matter which led to the name is not graphite, but a compound of carbon and iron. There is also a third cause, in the absence of any considerable compressive effect due to unequal contraction at the proper temperature for forging. How much this amounts to is problematical; Mr. Metcalf seems to rely upon such an effect to forge his castings, and doubtless it would benefit the steel if the casting could be cooled rapidly enough. But to cool a casting from the core out is not to cool it rapidly. To cool forgings rapidly they are quenched in oil. It is only necessary to glance at the relative proportion of cooling surface to mass in order to appreciate this. A gun-hoop 4 inches thick immersed in oil has one-half a square inch cooling surface to the cubic inch of metal, about. A cylinder 32 inches diameter, with 8¼-inch core, has .035 square inch to the cubic inch inside, of which the effect is available to its full extent, and .133 square inch outside, of which the effect is to be retarded in the process as proposed.

As to the effect of forging or "work" upon steel, I will state a law which may throw light on the subject under discussion. The effect of work upon the physical characteristics of mild steel is greater than of high carbon steel, and conversely the effect of treatment by heat upon the characteristics of high carbon steel is greater than of mild steel. Upon the medium steel of which guns are and should be made, whether by hooping or as the author proposes, both work and heat have potent effect.

Applications of the foregoing considerations to the hollow cast and subsequently treated gun, leads me to think that the conditions of Table No. 1, elastic limit 50 000 pounds per square inch inside and 40 000 pounds per square inch outside, are not possible without so great an increase of carbon as to render the steel entirely too "mercurial" for safe treatment or safe use. The moment it becomes necessary to lower the elastic limit inside, our margin of safety begins to disappear, or else powder pressure must be decreased.

It may be possible to attain elastic limit equal to that named in the new navy specifications for tubes at the expense of higher carbon, but the difficulties in treatment still remain to be overcome; indeed, the range of permissible initial tension becomes less.

It may be profitable to enumerate in the most general way some of the advantages which the system of building up guns has over that of casting them whole. They are:

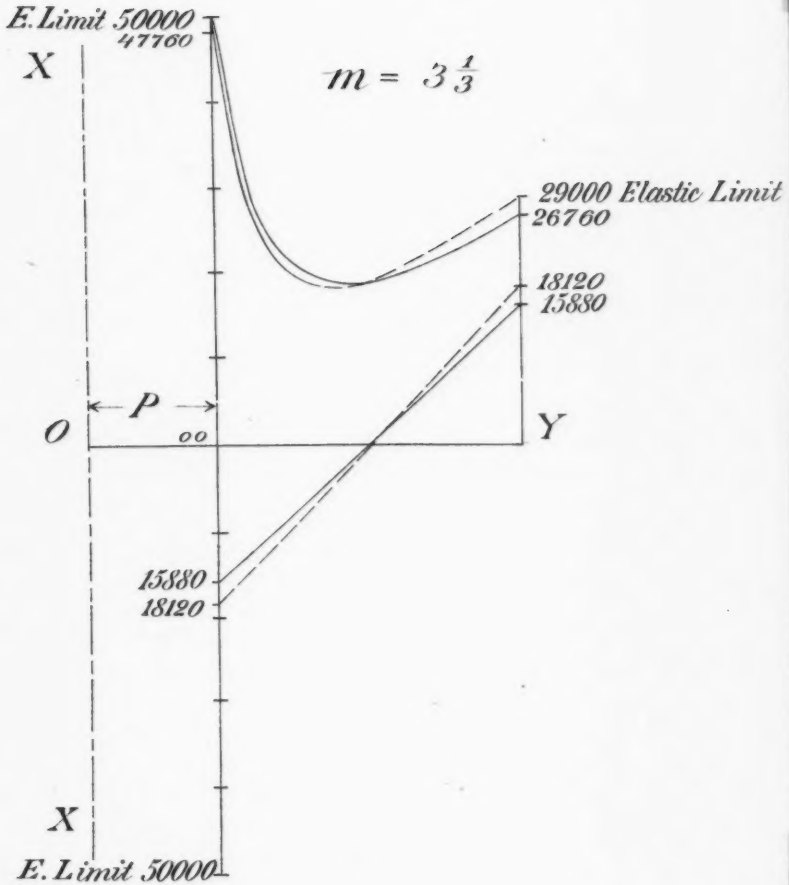
With metal of equal strength, greater ductility. With metal of equal ductility, greater strength. Metal of more reliability, because (1) the blow-holes, if any, have been closed up or got rid of; (2) thorough tests can be made.

Character of metal is known throughout. Character and amount of strains are known throughout and are controllable, so that two guns of same pattern are stressed the same.

To test this last statement, take the smallest diameter where definite shrinkage is required in the army 8-inch gun. It is 14 inches. Measurements of diameter to .0001 inch are quite feasible. Suppose we could measure no closer than .001, or with a probable error of .0005, then the error of stress due to this error of measurement would be $= E \div 28\,000 =$ say 1 100 pounds per square inch. I doubt if it is feasible to so thoroughly remove internal strains from a cast gun as to leave no part with an initial strain of that much, to say nothing of the possibility of putting on definite tensions within any such limit of error.

I do not wish to disparage the genius of Rodman, but the statement of the author in speaking of Wade and Rodman, that "none of their guns ever failed," will not bear investigation. In the Report of Senate Committee on Ordnance, 1869, is a record of the bursting of twenty-four Rodman guns of various sizes, nearly all at low numbers of fires, many at very low number; also of six such guns which burst or cracked spontaneously within the lathe or in the pit; also of many having been disabled short of actual bursting; also of many that were condemned for defects discovered after reception and mounting. The Rodman gun was an intelligent attempt to make a high-power gun out of weak material; we have much superior guns to-day, though very few of them. If the hollow cast (or solid cast) steel gun has a field, it is as a low power gun for commerce destroying or hasty inland defenses. In times of peace the policy of this nation being defensive, we do not want to invest money in commerce destroyers which would be useless in defending our ports, nor do we want to arm an expensive, swift cruiser, even though unarmored, with inferior guns, albeit the crusier would have to run away from an armored vessel. For coast defenses no guns but the best, because if ever used it will be against the best that the enemy has or can get.

Mr. William Metcalf.—In speaking of the effect of slow cooling producing large crystals and weak structure, Mr. Marshall ignores the fact that slow reheating to the right temperature will insure the formation of uniformly minute crystals and great strength, and that, having secured this condition, cooling from the interior, whether rapid or not,


E. Limit 50000
47760
X
m = 3 1/3
29000 Elastic Limit
26760
18120
15880
P
O
oo
Y
15880
18120
X
E. Limit 50000

lastic Limit

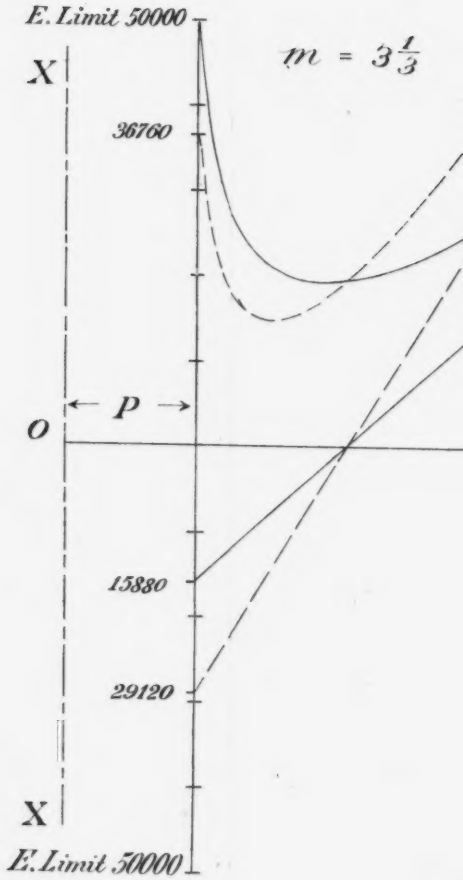
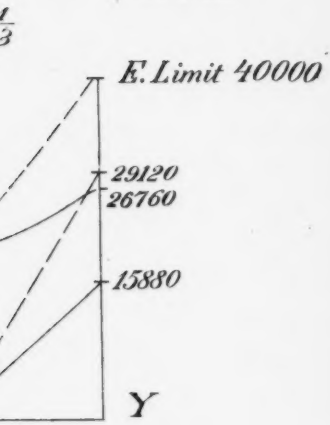


PLATE XXV
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI Nº 361.
MARSHALL
ON STEEL



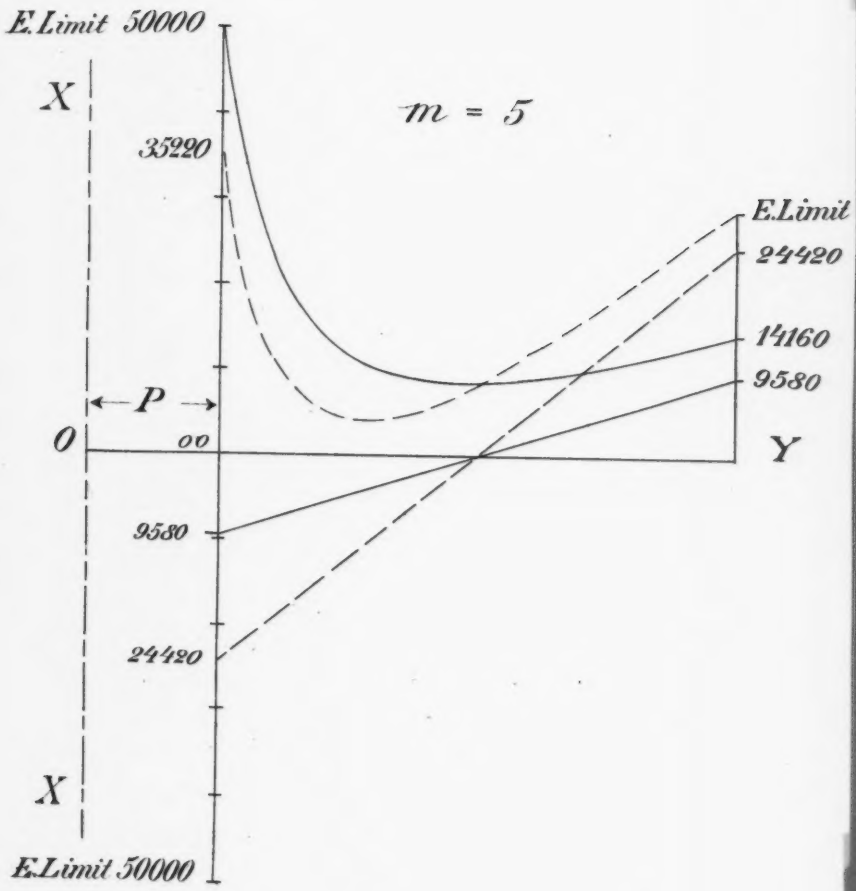
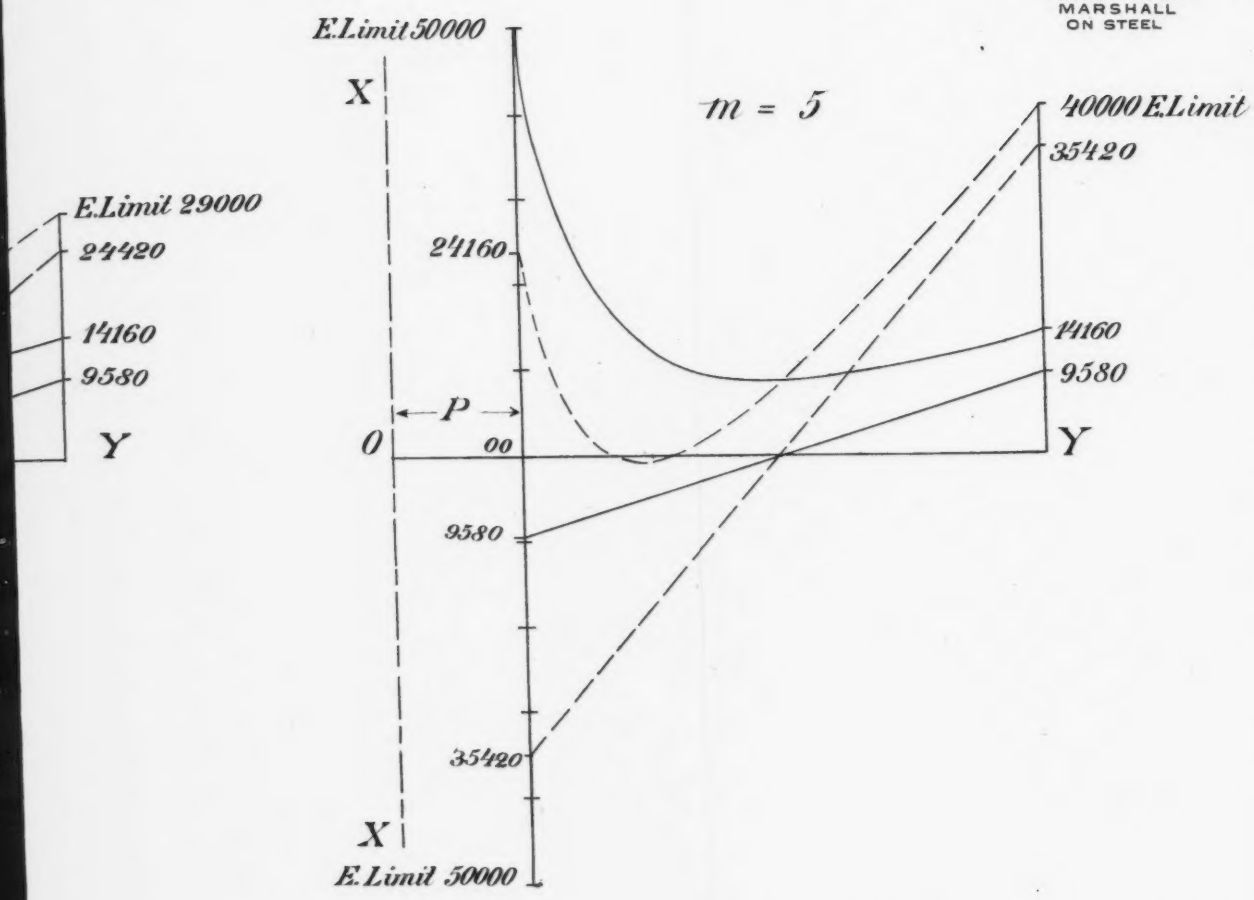

E.Limit 50000
X
m = 5
35220
E.Limit
24420
14160
9580
P
0
00
Y
9580
24420
X
E.Limit 50000

PLATE XXVI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 361.
MARSHALL
ON STEEL



will secure initial tensions in the direction desired. Heat is the true motive power with which to handle large masses of steel, and not steam.

Mr. Marshall says, "size very greatly affects the results of treatment by heat." This is an admitted fact so far as original heating is concerned; that is to say in cooling from fusion, but that has nothing to do with subsequent annealing, and I never suggested the use of castings without very careful treatment after casting.

Mr. Marshall speaks of "hardening" or "combined" carbon, and of a compound of iron and carbon. I decline to discuss this matter, because I have no faith in any opinion yet put forth as to the condition of carbon in steel. The whole question is subject to the Scotch verdict, "not proven."

As to the relative effect of work upon mild steel and high steel, I will accept Mr. Marshall's dictum in the absence of data, although it is contrary to my judgment, and to what might be called shop experience, which has not been verified by exact experiments.

I am still at a loss to understand what exact measurement has to do with definite shrinkage in the face of Mr. Marshall's admission that he cannot measure the temperature of orange color, and he might have said of any heat above the temperature of boiling water.

If a steel rod of ½-inch diameter be heated to a heat that is uncomfortable to the hand, say 120 to 150 degrees, it will enlarge its diameter .004 inch. Apply this to a gun ring 2½ inches thick and you would have a change .02 inch in thickness. A summer sun will do this and more for any gun ring; then what becomes of definite shrinkage and elastic limit?

The failures of Rodman guns mentioned were reported two years after I abandoned the gun business, and I had never heard of them. Of course I do not dispute the record, but I regret that Mr. Marshall did not give it to us in detail. The guns that "burst in the pit" have nothing to do with the question, unless the makers of built-up guns will report every defective ingot as a failure of a gun.

Mr. Henry M. Howe.—We must anticipate that gun steel, owing largely to its high melting point and the consequent enormous contraction which it undergoes after solidification has set in, as well as to the escape of gases during its plastic state and the consequent formation of blow-holes, will prove far more difficult to treat on the Rodman principle than cast-iron; and the difficulties which present themselves may indeed prove insuperable. Still, now that this principle has been successfully applied to cast-iron, and that the prevention of blow-holes is so well understood, the remaining difficulties hardly appear as formidable as those which Rodman conquered must have seemed before he attacked them. Mr. Metcalf's belief that they can be overcome is extremely encouraging; his opinion carries great weight, not alone be-

cause of his mental equipment and general knowledge of the subject, but also because of his special experience in mastering the similar difficulties which arose in applying the principle to cast-iron.

The advantages which would flow from the successful application of this principle to gun-steel would not be economy alone (though this is an important consideration, in spite of the silly sneers at cheap material which we hear; as if, forsooth, where quality is identical the dearer substance should be sought because it is dearer), but more especially because it would enable us to arm our frontier in a very much shorter time than would be possible had we to rely solely on built-up guns. A living dog is better than an unborn lion; how much more is a living lion! Even were a Rodman steel gun slightly inferior to a built-up gun, it would sink more ships than a not-yet-built-up gun.

In view of the advantages of economy and of rapidity of production, I believe that the chances of producing by the Rodman plan a steel gun equal, if not, indeed, superior to the best built-up gun, are sufficiently great to warrant the outlay required to settle the question by actual large scale tests.

The attempt to stop discussion by arraying great names like those of Whitworth, Krupp, Schneider and Fritz in favor of the built-up gun is unfortunate and futile. To-day we know how much expert knowledge is comparative, not absolute, and how much our most strongly entrenched opinions are liable to be dislodged. No; it is not the weight of names, but of evidence and argument, that settles the living questions of to-day.

Some experiments which I have just completed tend to throw some doubt on our data as to the strength of built-up guns. I understand that the strength of the members of a gun is ordinarily ascertained by examining test pieces cut from them, and by deducing the strength of the member from that of its test pieces. Now my experiments indicate that in this case the rule of three does not hold, and that the whole is by no means equal to the sum of its parts. In order to verify certain theoretical speculations concerning the effects of the stress due to suddenly cooling, I hardened several ¾-inch round bars of machinery steel of 0.39 per cent. carbon by quenching them suddenly in cold water from a white heat, under identical conditions. I found that the tensile strength of small test pieces cut from the interior and exterior of these hardened bars was exceedingly high, rising in one case to 248 000 pounds per square inch, while that of the hardened bars as a whole was less than half of this, or 118 000 pounds. This completely verified my deductions, which I shall shortly make public.

From this it is clear that test pieces cut from different portions of a gun-hoop may give misleading information as to the strength of the hoop as a whole. It may be demonstrated but it should not be assumed that annealing, as usually practiced, removes the conditions which create this difference between the strength of the mass and that of its test pieces.

ALFRED E. HUNT, M. Am. Soc. C. E.—The different grains in the structure due to the different temperatures and treatment of even the softest or 0.10 per cent. carbon steel that Mr. Metcalf refers to, are also noticeably shown in the tensile and bending results, as indicated by the following record of tests made by the writer.

A plate of soft open-hearth fire-box steel of $\frac{5}{16}$-inch thickness, and having carbon 0.12, manganese 0.36, silicon 0.008, and sulphur 0.017 per cent., was cut into strips $1\frac{1}{2}$ inches wide and long enough to get not only a test strength, but afterwards a bending test from an end allowed to project above the testing machine grips and not subject to strain, and prepared on the planer with straight edges that had all the effects of the shears cut off from them. These strips were subjected to both tensile and bending tests, enough of them in a normal state as the plate came from the rolls to establish the fact of the homogenity of the metal. Others of the strips were tested after being heated to various temperatures as follows: Some to a dark orange heat, some to a medium orange, bright orange, lemon, light lemon, low white heat, and scintillating white heat, at which the slag ran off from the metal. Some of the strips, after being thoroughly and uniformly heated to the temperature described above, were carefully annealed by allowing to cool down very slowly and uniformly in hot sand, and others allowed to cool off at the temperature of the air of the shop on a bed of blacksmith coals; and others still were, at the heats mentioned, plunged into water at 60 degrees Fahr., others into brine at 60 degrees Fahr., and others into oil at 60 degrees Fahr. A part of the quenched specimens in water were again heated to the various temperatures described above and then annealed by allowing them to cool down very slowly in a bed of sand which was at first nearly as hot as the strips, the object of these last tests being to indicate to what temperature the internal strains and hardened condition known to be in the steel due to the quenching would be best taken out in the subsequent annealing.

The results of the tensile and bending tests of these various specimens, after being treated as above described, are given in the following Table A.

My experience, not only from these tests given, but from many others obtained in the general way of my business, is that 0.10 per cent. carbon steel refines or anneals so that its grains are the most minute and most uniform in size at a temperature of about 1 000 degrees Fahr., or at a temperature just below that at which the scale raises on the metal; it appears as a medium orange color considerably below the lemon color. The length of time at which this heat has to be maintained varies according to the size of the piece to be annealed; it should surely be continued until the metal is thoroughly and uniformly heated through and through. Mr. Metcalf's rule that the heat best to harden at is the one at which it is the most thoroughly annealed, seems to be a good one; the tests given in the above table indicate this.

The crystalline character of iron and steel certainly undergoes a change due to temperature. Experiments to prove this have lately been made by Mr. Joseph Ramsey, Jr., Chief Engineer of the Cincinnati, Hamilton and Dayton Railroad, who will bring out the matter in an article to be read before the Engineers' Society of Western Pennsylvania.

Mr. Metcalf says that the engineer in preparing his specification for structural steel does well not to meddle with the chemistry of the metal at present. From his qualification of "at present," and his acknowledgment that the purer the steel is to the two elements, iron and carbon, the better it is, the inference can be fairly drawn that his reason for this is that our knowledge is too limited as to the effects of the various chemical constituents, and our power of producing the same physical results, even with the same chemical composition, is too limited to specify, in addition to the physical requirements, a prescribed chemical constitution as well. In this I would agree, except as to phosphorus, which is a well known and acknowledged element of weakness to structural as well as other steel. While I would cheerfully concede the fact that well authenticated sample specimens of steel have been produced with phosphorus of over 0.20 per cent. which have shown, with very low carbon, all the ductility requirements of the best structural material, and that upon the results have been based many bright and golden hopes, especially by the advocates of the Clapp-Griffith steel process, yet actual experience has clearly shown that these have been simply isolated cases, and they have only caused the expenditure of much money in experimental work with results so far of only signal failure to produce a uniformly ductile steel high in phosphorus. The fact that such results have been obtained, indicates so far only possibilities in the future metallurgy of steel. For my own part I am clearly of the opinion that Mr. Metcalf's statement should be reversed, and that at present it is extremely important to stipulate the maximum contents of phosphorus. This is needful, because in the ordinary structural steel as made to-day, the presence of this element gives rise not only to cold shortness or brittleness, but also to irregularity in the metal. This is needful also because the cost of production may be materially lessened by even so small an increase in the steel as one or two one-hundredths of one per cent. of phosphorus above, say, 0.06 or 0.08 of one per cent., this being a well-approved maximum limit for many grades of structural steel. Just in the same way that iron ore is valued and sold by "the unit," or by the percentage of contained metallic iron, so the stock from which steel is made, is valued and sold according to its freedom from "units" of hundredths of a percentage of contained phosphorus. The amount of sulphur, and the impurities tending to produce red shortness, can very properly be left to the manufacturer, as his own interest in the matter of producing sound steel free from surface defects will regulate the content of these elements. The percentage of carbon and manga-

nese ought not, except within reasonably wide limits, to be stipulated as varying stock, with even the same chemical composition, requires varying proportions of carbon, the chief hardener, and of manganese, the antidote and purifier, to produce the same physical results.

I agree with Mr. Metcalf most cordially in his advocacy of the Rodman principle for the manufacture of steel guns, and would give below some results of tests made from specimens cut both lengthwise and transversely from forgings made for two of the first 6-inch built-up breech loading rifles for the U. S. Navy Department at two different steel-works where the writer had charge of the work. The results given in Table B show what Mr. Metcalf has so ably pointed out, that the temperature at which steel was last subjected, moderated by its subsequent treatment, is always recorded in the structure of the steel, and that the annealing is one of the most important of all metallurgical operations to which the steel is subjected from the stand-point of the engineer.

It is also worthy of note that for shafting and for many of the purposes where large forgings are now required, that both the strength and ductility of steel can be obtained by piling and welding steel fagots after the same general methods by which iron forgings are made. To accomplish this, it will be needful to cast the steel in large 16 by 18-inch ingots of approximate composition, carbon 0.16 per cent. and manganese 1 per cent. The high manganese materially hardens the steel and gives it also a welding property, and in this steel the sulphur and phosphorus should be extremely low. These large ingots should be rolled in heavy trains into bars of 6 to 8 inches width by 1 inch thickness, and to a length a few feet greater than that to which the forging is to be completed. The bars may then be bound together and brought up, a part of them at a time, to a good welding heat. They should then be hammered until perfectly welded in the same way as with iron, and then drawn out by forging to the required finished shapes in swage-shaped dies, and afterwards given a careful annealing. Steel of this composition, high in manganese, will weld in skillful hands just as readily and securely as wrought-iron. In this way steel forgings can be made with the crystals of the metal fine and uniform in grain, this texture having been secured by the large reduction of area, by the rolls, from the ingots to the bars. This pile can thus be worked under the hammer much more effectually than the large and unwieldy original ingot from which otherwise the forging must necessarily be drawn out. It is extremely difficult to heat uniformly a single ingot as large as would be needed for a heavy forging if hammered from the ingot direct. It is very doubtful if, under ordinary conditions of working, blows of the heaviest hammers can be made to penetrate effectually such large ingots. On the contrary, the benefit due to the reduction in rolling of the ingots to the small bars is felt throughout the entire mass of the forging when thus built up. In forgings, especially those subjected to torsional strains, if each weld be a good one, it is the judgment of the writer that, in respect to strength, they are by no means a disadvantage in such a crystalline structure as steel.

TABLE A.

Description of Treatment of Specimen.	Tensile strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Percentage of elongation in eight inches.	Percentage of reduction of area.	Character of fracture.	Bending Test.
						Degrees.
Normal specimen, as plate was rolled and cooled	56 100	32 210	29.20	58.80	Silky	180 on itself.
" " "	55 320	32 190	28.70	59.60	"	180 "
" " "	56 920	32 640	29.50	62.40	"	180 "
Heated to a dark orange	58 400	32 440	29.90	60.80	Silky	180 on itself.
" medium "	62 100	32 600	30.40	61.40	"	180 "
" bright "	60 140	31 450	29.10	59.40	"	180 "
" lemon	57 190	30 100	27.60	54.10	"	180 "
" light lemon	54 400	37 990	24.00	46.30	Amorphous	180 " cracks.
" low white	56 100	36 400	21.10	49.30	"	180 " "
" scintillating white	43 980	37 190	2.10	20.20	Fiery, large crystals....	90 breaks.
Annealed in sand.						
Heated to a dark orange	58 190	31 850	29.20	59.10	Silky	180 on itself.
" medium "	59 150	32 790	29.90	62.30	"	180 "
" bright "	60 010	32 950	30.50	61.10	"	180 "
" lemon	57 110	31 430	29.20	59.20	"	180 "
" light lemon	55 330	30 260	28.50	57.50	"	180 "
" low white	50 100	30 050	27.20	55.50	"	180 "
" scintillating white	40 150	38 580	2.10	4.50	Fiery, large crystals ...	60 breaks.
Allowed to cool at temperature of air on bed of coal.						
Heated to a dark orange	72 590	44 430	21.10	50.50	Silky	180 on itself.
" medium "	73 980	45 580	20.20	47.20	Amorphous	180 "
" bright "	59 110	44 760	21.80	46.10	"	140 breaks.
" lemon	69 840	44 590	21.10	46 50	"	130 "
" light	80 330	50 210	16.20	34.60	Fine crystalline.....	110 "
" low white	82 110	51 210	10.10	35.30	" "	100 "
" scintillating white	41 160	38 210	2.40	3.30	Fiery, large crystals....	50 "
Plunged in water at 60 degrees.						

Heated to a dark orange	82 460	55 560	16.10	40.30	Fine crystalline	180 on itself, slight cracks.
" medium "	85 790	58 460	12.90	38.30	" "	180 " O. K.
" bright "	89 310	60 490	15.30	39.20	" "	180 " cracks.
" lemon	86 460	61 150	14.50	38.70	" "	130 breaks.
" light lemon	89 840	63 580	13.90	32.10	" "	120 "
" low white	82 060	59 470	15.80	28.70	Crystalline	90 "
" scintillating white	42 490	37 560	1.80	1.90	Fiery, large crystals	50 "
Plunged in brine at 60 degrees.						
Heated to a dark orange	64 110	35 470	24.80	53.50	Silky	180 on itself.
" medium "	67 230	37 590	25.60	50.10	"	180 "
" bright "	66 490	36 470	22.30	50.10	Fine crystalline	180 " cracks.
" lemon	69 460	39 510	18.10	47.70	" "	160 breaks.
" light lemon	76 830	40 460	15.60	41.50	" "	150 "
" low white	78 850	46 310	14.30	31.20	Crystalline	130 "
" scintillating white	39 760	38 670	0.75	0.75	Fiery, large crystalline	45 "
Plunged in oil at 60 degrees.						
Heated to a dark orange	59 160	31 450	29.75	59.50	Silky	180 on itself.
" medium "	62 490	32 650	31.20	62.40	"	180 "
" bright "	61 859	32 700	30.50	60.40	"	180 "
" lemon	60 190	32 120	26.70	57.20	"	180 "
" light lemon	61 370	35 670	24.80	54.30	"	180 "
" low white	59 210	30 490	20.70	48.10	Crystalline	180 cracks.
" scintillating white	37 860	36 870	1.00	1.80	Fiery, large crystals	45 breaks.
Annealed in sand after first being heated to a lemon color and quenched in water.						

TABLE B.

Description of Test.	Tensile strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Per cent. elongation in 2 inches.	Per cent. reduction.	Character of fracture.	Bending test around a 1-inch pin.
						Degrees.
Gun No. 1.						
Transverse test, cut from a disk from jacket	81 100	50 110	11.80	.....	Coarse crystalline	110 broke.
Same, annealed	84 210	50 990	24.70	.....	Fine "	180 "
Same, tempered in oil	127 480	88 860	16.10	.....	" "	160 "
Transverse test, cut from a disk from the plug	71 840	42 470	12.70	.....	Coarse crystalline	140 broke.
Same, annealed	74 310	47 760	28.60	.....	Fine "	180 "
Same, oil tempered	90 270	59 470	17.10	.....	" "	150 "
Transverse test, cut from a disk from the tube	64 440	34 460	16.30	.....	Coarse crystalline	130 broke.
Same, annealed	68 800	37 710	26.30	.....	Fine "	180 "
Same, oil tempered	84 350	54 970	19.10	.....	" "	180 "
Gun No. 2.						
Plug made from an 18-in. square ingot of.. {Carbon .26 {Manganese .35						
Tube " 29-in. diam. " .. {Carbon .25 {Manganese .51						
Jacket " 47 " " " .. {Carbon .33 {Manganese .66						
Plug, 9 in. diam. Tube, 10½ in. diam. Jacket, 18½ in. diam.						
Disk at breech-end of gun tube, transverse test	67 450	38 100	16.60	21.1	Coarse crystalline	130 broke.
Same, annealed	69 580	39 110	28.30	58.0	Silky	180 "
Same, tempered in oil	88 350	51 150	24.10	47.6	Fine granular	180 "
Disk at muzzle-end of gun tube, transverse test	66 210	35 240	15.30	19.1	Coarse crystalline	140 broke.
Same, annealed	68 880	39 880	29.99	59.4	Silky	180 "
Specimen cut longitudinally from butt-end of gun tube	70 110	40 440	17.40	21.8	Coarse crystalline	120 broke.
Same, annealed	71 000	40 790	31.80	60.3	Silky	180 "
Same, oil tempered	99 210	70 690	20.10	41.4	Fine granular	180 "
Specimen cut longitudinally from muzzle-end of gun tube	69 350	38 840	14.20	18.8	Coarse crystalline	120 broke.
Same, annealed	70 240	40 410	32.70	53.9	Silky	180 "
Same, oil tempered	94 990	60 210	20.70	23.3	Fine granular	180 "
Disk cut from jacket, transverse test	82 210	50 140	12.10	15.5	Very coarse crystalline	100 broke.
Same, annealed	86 660	52 230	26.00	44.3	Fine granular	180 "
Same, oil tempered	115 990	69 390	18.30	24.8	" "	180 2-in. pin.
Disk cut from plug, transverse test	72 880	40 360	13.40	17.6	Coarse crystalline	140 broke.
Same, annealed	77 490	48 390	29.90	41.3	Fine granular	180 "
Same, oil tempered	99 990	61 140	19.90	26.6	" "	180 "

Mr. WILLIAM METCALF.—Mr. Hunt, in advocating the conversion of steel into muck bar to get the grain fine, and then piling, heating and welding the same, ignores the fact, so clearly shown in his own paper, that this welding heat will reproduce "fiery, large crystals."

I hope no engineer will adopt Mr. Hunt's suggestion, for the simple reasons that steel cannot be welded safely by any known process, unless it be the new electrical method confirming Mr. Howe's experiment.

A bar of steel about six inches diameter and forty feet long, broke at a strain of 36 000 pounds per square inch. The manufacturer bored a sample out of the fractured end, and it showed over 100 000 pounds tensile strength. Subsequent bars for the same work were made to meet all requirements, presumably by annealing the bars after they were finished.

I did not say nor intimate that a steel cast gun could be made for 9 cents a pound; I said good castings can be bought for less than 6 cents a pound. It is probable that steel cast guns would cost 20 to 30 cents a pound finished weight.

Senator JOHN T. MORGAN (by letter).—I had only one reason for desiring you not to mention my name in connection with the theory of the fluid mixture of iron and steel. It was that I dislike to advance theories that are aside from studies or topics that belong to my line of professional effort. I think the fact important, and very clear, that iron is a fluid. Its importance relates chiefly to the question of the best methods of purifying it and making it tough and hard. The change in the molecular structure of iron by lapse of time simply, without any discernible extraneous force, is due to its fluid character, an automatic faculty of rearranging its crystals. We know that many disappointing and even tragic results have grown out of this change in the structure of iron and steel which has escaped the most accurate observation.

If the fluid theory should enable us to anticipate these changes even, and to provide against their serious consequences, I will be very proud of having thought about a matter of such importance, however imperfectly.

As the subject is now up for examination amongst the thinkers who are deeply studious on such matters, I should feel honored to have it said of me that my mind had been drawn to the conclusion that iron is a fluid, by those simple processes of reasoning that even the faith of men who have no pretension to being scientists.

Professor JOHN W. LANGLEY.—In response to the invitation to join in the discussion of Mr. Metcalf's paper on "Steel," I will try to touch briefly on one or two points connected with the chemical and physical side of the subject.

IS STEEL A FLUID?—Engineers have become familiar of late years with the conception that certain metals do behave, under stress, like very

viscid fluids. Under mechanical tests the evidence is very strong that they are such. Is there any proof that chemically they behave like fluids rather than solids? I believe that so far as iron and steel are concerned the answer can be given in the affirmative.

First.—In mathematics we may regard the straight line and the circle as being each "particular cases" of the ellipse; and as between these extremes we may have ellipses of all possible ratios between the major and minor axes, so by analogy I consider we may have all possible grades of fluidity between a typical liquid, like water, and a typical solid, such as a single crystal of the diamond or quartz, or alum. These latter bodies do not permit of any displacement of adjacent parts (beyond their limit of elasticity) without rupture; they do not yield gradually to a continuous stress; and they will not, when overstrained, take a "new set." Steel will, however, do all of these things under a suitable pressure.

Second.—All liquids have the property of "surface tension," and so also do most of the industrial metals show the phenomenon of the "skin," or abnormal state of the superficial layers of molecules. If this "skin" is carefully dissolved away by dilute acids, a piece of metal, such as a coiled spring, will change its shape visibly during the action of the acid, but true solids in single homogeneous crystals do not thus change shape by twisting or bending when they are gradually attacked by a solvent.

Third.—The removal of the state of surface tension, or its modification, as by putting oil or alcohol into water, causes currents and molecular movements throughout the entire mass of the fluid till a new equilibrium is established.

Now it appears to me probable that the profound change in the texture and tenacity of the wires in my laboratory, referred to by Mr. Metcalf in his paper, was due to the change on the surface caused by an exceedingly thin film of corrosion due to an atmosphere slightly impregnated with acid vapors. Under the static stress thus set up by altering the "surface tension," the vibrations of the building, extending over three years, were sufficient to rearrange all the molecules of the wire into new forms, and an extreme brittleness was the result. Many cases of the so-called crystalline alteration of car-axles, etc., I think may be traced fully as much to external corrosion as to vibrations. May not both be necessary? Experiments in this line are now in progress.

Fourth.—Chemists, by researches on the heat equivalents of chemical actions, have been led, somewhat generally, within the last few years to consider that there is no philosophical difference between combination and solution; that, broadly considered, solution is a particular case of combination. I have shown, Proceedings American Association for the Advancement of Science, 1883, that even such important decompositions as sulphuric acid from sulphates of lime, copper and iron, could be produced by the simple solution of these salts in pure water.

Is it not, therefore, to say the least, a good working hypothesis to regard the carbon, phosphorus, etc., in steel as being present in solution and not as definite carbides. And just as a salt is partly extracted or crystallized from a hot solution on cooling, so we know that graphite is extracted from white iron by slow cooling. If a definite carbide of iron exists in pig metal we ought to obtain in gray pig crystals of the carbide, but instead we obtain, practically, plumbago.

In conclusion, let me say that a working hypothesis is valuable only as it accords with practice. Is it not probable that many of the well-known peculiarities of iron and steel can be explained and industrially dealt with, with some measure of success, in proportion as we treat the problem as essentially the problem of a viscid fluid.

I take pleasure in saying that during a period of fifteen years in which I have had professional knowledge of Mr. Metcalf's work and views, it has seemed to me that his mechanical and industrial results were not in opposition to any chemical data within my possession.

Professor JOHN W. LANGLEY (at a subsequent meeting). The following reference I was unable to obtain when writing on the discussion of Mr. Metcalf's paper.

As evidence that the carbon in cast-iron and steel is held in a different manner to what it is in manganese and ferro-manganese, see *Comptes Rendues*, page 964, Vol. LXXX, April 12, 1875, for a paper by Messrs. Troost and Hautefeuille on the "Heat of Combination of Carbides of Iron and Manganese."

In this paper the authors show that the heat of combustion of the iron carbides is much greater than that of the manganese carbides and they conclude: "Carburized irons are formed by absorption of heat, starting from their elements. This fact classes these irons in the category of explosives or of solutions."

The manganese carbides they say are analogous to definite chemical salts, being formed with marked evolution of heat, starting from their elements, hence are not solutions.

Mr. M. J. BECKER, M. Am. Soc. C. E.—After Mr. Metcalf's emphatic indorsement of the use of cast-steel for heavy ordnance, the answer to the question which I propose to ask him may perhaps be considered a foregone conclusion. Still, if there is any one question in the discussion of which one may expect to be startled by unexpected revelations, it is this very subject involving the physical properties of steel. In the construction of modern bridges, the general tendency has been of late to abolish entirely, or to reduce to the most unimportant details, the use of cast-iron. There have been abundantly good reasons for this. Aside from the general uncertainty of the product, the inferior facilities for casting by the older methods, have made it very difficult, if not impossible, to produce columns of uniform thickness and density;

and when such imperfectly cast members were used in lighter structures, requiring but small sectional areas, any little flaw or inequality would very seriously impair the strength of a small sized piece; and although there have been comparatively but few disastrous results from this cause, the existing danger has been fully realized and has led to the abandonment of the use of cast-iron in general bridge construction. In larger structures, with members of greater sectional area, this danger from inherent imperfections would, of course, not exist to the same extent as in lighter structures with smaller members; but when the confidence in the virtue of the metal was once shaken, the general distrust applied to all shapes and sizes alike. I have noticed, however, quite recently, that some of the leading bridge-builders in this country are returning to the use of cast metal in large structures, and employ soft, malleable cast-steel for such parts as the large bolsters or shoes for the footings of the end columns of bridges of 500 feet span, which require not only metal of great thickness of bearing surfaces, but large sizes generally, with many interior strengthening ribs and webs for the support of pins as large as seven inches diameter. These shoes must be cast whole, and the metal must be of a quality which admits reaming and turning of the large pin-holes, and the planing of the bottom plates for the movement of the friction rollers.

I would ask Mr. Metcalf whether he would recommend the use of cast-steel for such purposes, and if so, what quality of steel he would prefer, considering, of course, not only the static loads to be carried by the castings, but also the constant shocks and vibrations of the moving traffic to which they must be subjected.

Mr. WILLIAM METCALF.—Yes, use dead soft steel annealed carefully; never without annealing. Do not mind the blow-holes, they indicate mild steel; but be very careful to avoid sharp angles and complications of unnecessary ribs. Always study simplicity of form, and fillet thoroughly every corner.

In many ways I find in our works that the good, reliable, staying qualities of good steel castings form the chiefest comfort of my shop life. We don't make steel castings, but we use them.

Professor WILLIAM H. BURR, M. Am. Soc. C. E.—Although not bearing directly on Mr. Metcalf's interesting and instructive paper, it may not be out of place to note a point brought out by late specimen tests of structural (mild) steel.

It is and has been common practice to put into long span bridge columns steel with an ultimate tensile resistance varying from 65 000 to 80 000 pounds per square inch, and to subject the columns to working stresses proportionally increased (or at least approximately so) over those for wrought-iron columns similarly placed. It is both interesting and important to observe that the most recent compressive tests on specimens

of mild steel justify these increased compressive working stresses for short steel columns, *i. e.*, for columns under perhaps 175 radii of gyration or 65 diameters in length if with pin ends, or still longer under other end conditions. Below these limits (where are found, of course, all bridge columns) the limit of elasticity governs the strength of the column, while above them the coefficient of elasticity is the basis of column resistance. There is no well defined line between these two fields of resistance, so to speak, as they shade gradually into each other, but the distinction is important, for it holds in full value for the columns used in bridge construction.

Specimen tests show that 65 000-pound tensile steel possesses a compressive elastic limit of about 40 000 to 42 000 pounds per square inch, while 80 000-pound tensile steel possesses an elastic limit of not far from 45 000 to 48 000 pounds per square inch in compression.

When these results are compared with a 26 000 or 28 000-pound elastic limit for wrought-iron, it is reasonable to infer that the working stress in even 65 000-pound (tensile) steel columns may be safely increased from 33 to 50 per cent. over that used for wrought-iron under similar circumstances.

It is also reasonable to infer that tests of full-sized columns, properly designed and built, will verify these conclusions, although published results of such tests are still lacking.

Regarding the cast-steel gun question, a layman should undoubtedly speak with great caution; he may, however, properly regard the matter as more experimental in character than Mr. Metcalf seems to. If a cast-steel gun can be produced for nine cents per pound, equal to the performance of the same duty, pound for pound, and equally reliable in its finished state to that which costs seventy-five to eighty cents per pound, there is little doubt which should be built. But there are serious doubts in the minds of many people whether Mr. Metcalf's estimate may not be a little low. In fact it could be more readily accepted as final if cast-steel guns of the heaviest caliber had already been fabricated at that price. The difficulties attending such a production have not yet been met, and the difficulties are seldom over-estimated under circumstances similar to these. If indeed such excellent results can be so confidently anticipated, it would seem to be a most natural thing for cast-steel manufacturers to produce at least a few small guns, and subject them to the proof of the severe firing tests which built-up guns are constantly sustaining.

Even with the increased price per pound at which a few only of such guns would be produced, the cost of the operation, in view of the profitable business sure to follow, would be a small consideration.

The experience of engineers with ordinary steel castings does not seem to me to justify confident expectations regarding the reliability of a gun produced by casting from steel with internal cooling. I am of

course aware that the metal put into guns would not be precisely the same as that used for ordinary castings, but it is a very serious question in my mind whether such a mass of steel with no forging either in the liquid or solid state (for cooling is not forging) would be even approximately homogeneous.

I do not assert it would not be, but in the absence of such a gratifying result as a large homogeneous steel casting free from gas-fissures or bubbles, I do not believe that positive predictions on that side of the question can be at once accepted.

In reasoning from cast-iron guns to cast-steel, it also seems to me that difficulties are not altogether avoided. In the first place, it is not an exaggeration to say that a gun of given caliber, in order to meet the requirements of modern ordnance, must do double the duty of the same gun under similar circumstances twenty-five years ago. Hence any fault in the material due to any part of the process of fabrication will result in far greater relative and absolute damage to the gun. If therefore the process of casting and cooling should prove to be more delicate and difficult to control with steel than with iron, as would probably be the case, the resulting product would be of most doubtful value.

It seems to one not an expert in ordnance material, that the first evidence of the fact that the requisite steel, with proper degree of homogenity, high elastic limit and toughness, can be fabricated without forging either in the liquid or solid, or semi-solid state, is yet to be produced.

Hammer-forging may be damaging to the material, but hydraulic forging is not, nor is forging in the liquid state, such as is obtained under great pressure by the Whitworth process. It certainly is a fact that the best and most highly effective modern ordnance has been manufactured under processes involving those operations, with subsequent hardening and annealing. If the process of building up guns places the right kind of metal in such position and condition as to best resist the high intensities caused by firing, it certainly cannot be called unscientific. But if better results can be obtained at less cost by casting and internal cooling, by all means let us have them. Thus far, however, we have them on paper only, and that step is not a long one.

Mr. A. GOTTLIEB, M. Am. Soc. C. E.—The subject of Mr. Metcalf's paper is of such character and general interest that it inspires participation in its discussion.

I am sorry to be prevented from attending the meeting and taking part verbally in the discussion, being therefore compelled to confine my remarks to a few of the points touched by Mr. Metcalf.

That gentleman, through his long connection with the manufacture of steel, and by the great attention and close observation he has devoted to the subject of steel, in its manufacture, treatment, and the various uses of it, is an expert on this subject whose views and recommendations are bound to command due consideration.

Nevertheless there may be some points connected with this matter which may be of importance enough to be dwelt upon more than Mr. Metcalf has done, for one reason or another, or which may have come more frequently to the notice of others. To begin with, I cannot agree to the definition of Mr. Metcalf, or of United States Senator John T. Morgan, that iron or steel is a liquid, no matter how exhaustive and able the argument in favor of that theory may be. We have to consider the material as it is when ready for use in its ultimate shape, and the qualities it then possesses. For the same reason we make a distinction between water, steam and ice; although the substance is the same, the forms and qualities are different.

The qualities, at least the physical ones, of molten iron or steel are about the same; there is not much difference between them, except perhaps their density or specific gravity at the same temperature. How widely different are they in the concrete form.

It may perhaps be called putting the cart before the horse when I state right here that, in my opinion, steel is, and always will be, a treacherous material. Without much argument, and without citing my own experience, Mr. Metcalf's paper itself is proof enough for this assertion.

Any material that through a few degrees of heat more or less; through unequal heating or cooling in the same piece; through a blow or scratch on its surface, may fail to perform the duty in critical moments that we reasonably may expect from it, is dangerous, no matter how good it may be if perfect. The condition of perfection is dependent from so many factors, that it is next to impossible to have them all under our control. I agree with Mr. Metcalf and others that mild steel will stand more abuse than hard steel, and perhaps more than iron. But the term mild steel is only a relative one. What Mr. Metcalf may term mild steel from his stand-point, using steel of 1.00 and 1.2 carbon, other engineers using steel for structural purposes may call high steel. Mild steel capable of resisting hard treatment in pulling, punching, shearing, bending, etc., cannot have more than 0.10 to 0.18 carbon; steel of 0.20 to 0.35 carbon of open-hearth or Bessemer process belong already to the high grades of steel for structural purposes. These latter are still mild steel compared with tool steel, but have already all the unpleasant qualities that render their behavior in heating and hammering as uncertain as the highest grades. I will only remark that while the lower grades of steel will stand more abuse than iron under certain conditions, the good qualities can be easier destroyed, by improper heating, than in iron, and generally the physical qualifications are not much better than those of good iron. The cost of production of mild steel may under certain circumstances be less than that for good iron, but there is no large gain in strength over iron by using it.

The higher the grade, or the more carbon the steel contains, up to

a certain limit of course for every specific purpose, the more advantage there is to use steel, provided the dangers inherent to such steel can be eliminated.

Equal heating and proper annealing are amongst the foremost points recommended by Mr. Metcalf and others, and by right so. But how difficult is it to have these two operations performed exact in each case.

Mr. Metcalf's example of the piece of steel of only 0.10 carbon (really soft wrought-iron) heated to different degrees and cooled in water, illustrates sufficiently the dangers of unequal heating. In higher carbon steel the difference of heat, to bring about such result, will be considerably smaller than here stated. To equalize such strains produced through unequal heating, it has been generally recommended and adopted to anneal such pieces after their manufacture. In my experience I found that mild steel of 0.10 to 0.18 carbon requires annealing to equalize such strains, for instance, in eye-bars for bridges, when one part of the bar has been heated in the fire and hammered, while the balance was left cold. I found, however, that every annealing diminishes the original amount of carbon in the bars and reduces the ultimate strength in tensions from 2 to 5 per cent. of the original strength before annealing.

Uniformity of the material is the first requirement, no matter what the other qualities may be. To obtain this, equal heating of all parts and annealing is recommended and applied. But there is another just as important an operation, which is usually neglected, and which Mr. Metcalf seems to touch only slightly. Next to the equal heating is the equal cooling.

While rapid cooling produces small crystals, and consequently stronger steel than slow cooling, it may be advisable to dip the hot steel of small size into water or oil. For steel of large masses, like guns or armor plates, I consider this kind of dipping or cooling not only useless, but dangerous. The outside parts of these masses must necessarily be cooled and changed in their structure before the inner parts are reached, which condition in itself must produce unequal strains in the great mass. For such large masses, in my opinion, the proper cooling is that which starts in the center of the mass, keeping the outside from too rapid cooling by external heat, until gradually the whole has cooled uniformly. Even at the risk of producing larger crystals and smaller ultimate strength, I would prefer such a process that would produce more uniform material. In this connection I would mention that, from my observation, hot rolled steel bars placed upon the cooling bed in the mills, one end nearer to the cold outside air than the other, or one side near the cold floor, show entirely different fractures. This would indicate that it would improve the uniformity of steel rails and plates, by cooling them under cover, instead of exposing them to drafts and cold outside air.

This rapid local cooling of certain parts of the same piece causes

also the small surface cracks, which may become so dangerous, as mentioned in Mr. Metcalf's paper.

In conclusion I would like to state that, from my observation and experience, I believe myself to be justified in saying, without in the least wishing to deter any of the merits of the ideas and results obtained by General Rodman or Captain Michaelis, and hesitating to take issue with such a recognized authority as our late Mr. A. L. Holley was, that the pressing of steel by hydraulic pressure, and for large masses, preferably the hammering, is not so cruel a process as Mr. Metcalf represents, provided the large masses are properly heated, the hammering or pressures large and strong enough to finish the operation while the steel is not cooled beyond a proper limit; and when the operation is finished the proper means are on hand to have the piece either annealed or cooled in the right manner. I believe under such circumstances the steel is improved by hammering or pressing.

That the product so obtained is costlier than castings would be, is true beyond question, and for this reason it would be well if the United States Government would institute experiments to demonstrate the comparative merits of the various processes as Mr. Metcalf suggests.

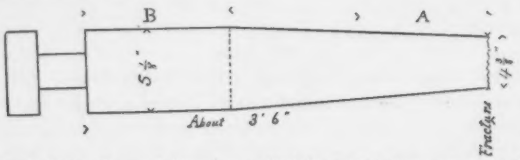
Mr. Percival Roberts Jr., M. Am. Soc. C. E.—The perusal of Mr. Metcalf's paper cannot but impress the reader as the utterances of one who is a thorough master of the subject of which he treats. Leaving the question of gun steel, which really is, considering its tonnage, but a very small item to the annual total consumption of steel, we come to Mr. Metcalf's remarks in reference to the manipulation of steel during manufacture for structural purposes, and I believe no truer words were ever spoken than when he says: "Heat is the power which gives to steel all of its good and all of its bad conditions."

I do not understand by this that steel bad originally will be made good by heat, but that good steel can very readily be made bad by the same. If the above theory is accepted as true, do we in our present practice sufficiently test steel in the finished shape in which it is to be used in our structures, or do we not rather give too great an importance to test of a form under conditions entirely foreign to those existing in the finished product? I refer to the testing of ¾-inch round rolled from either large or small sample ingots. There seems to me to be no more reason for testing a sample ¾-inch round and then accepting the heat of steel than there would be in testing the pig iron before it went into the furnace or cupola. The engineer and the manufacturer each have a certain territory, the bounds of which I think they should not overstep. To the former belongs the right of making all specifications as to quality, either physical or chemical, to which he may wish his steel to conform. He may also, if he sees fit, decide by what commercial method his steel shall be made.

To the manufacturer, on the other hand, should be left the manipulation of the material, free from all interference whatever, until it is delivered in its final form, and not until then is it the duty of the engineer to test or inspect. I think it possible in the future that arrangements can be made and systems perfected to make as full and complete tests of material in its finished form as will satisfy the most critical; not necessarily in a machine for tension, etc., but probably a certain number of each and the balance by bending or otherwise, and each shape or section thus accepted or condemned on its own merits, for which good steel may be stronger than good iron. Bad steel is infinitely worse and more dangerous than inferior iron, and the material is one which must be handled during all processes with intelligent care.

Let us suppose, however, that due attention has been given to tests; are we certain that the testing machine and other various specimen tests will give us the proper and necessary information as to the suitability of steel for structural purposes?

A short time since in filling an order for steel axles to conform to the Pennsylvania Railroad tests and requirements, a lot of Bessemer blooms were used, procured from a reliable maker and furnished under his guarantee to meet the above specification, viz.: The finished axles being $4\frac{3}{8}$ inches diameter at the center were required to stand five blows of a drop weighing 1 640 pounds falling 25 feet, the axle to be placed upon bearings three feet apart and reversed after each blow. Of ten axles tested only one met the requirements, the balance failing at from two to four blows. Three of these axles were then taken and from them test pieces were cut as below:



The test piece *A* was taken immediately adjoining point of fracture, and *B* as far removed from same as possible. The results were as follows:

	E. L.	B. S.	Stretch per cent. 8 inches.	Reduction per cent.	
Axle No. 1 *A*....	36 940	62 100	18.7	48	All per square inch.
" 1 *B*....	36 580	64 020	20.2	27	
" 2 *A*....	32 680	58 500	27.0	50	
" 2 *B*....	32 400	59 150	26.5	50	
" 3 *A*....	36 300	63 060	22.0	47	
" 3 *B*....	36 360	61 820	21.5	45	

Axle No. 1 stood the required drop test unbroken.
" 2 failed at the second blow.
" 3 failed at the fourth blow.

Again, in another instance, four axles made from the same lot of open-hearth steel containing .20 carbon and charged in the same heat at the forge, gave under the drop the following results:

Axle No.	1	broke at	48	blows.	1 640 pounds falling 25 feet.
"	2	"	2	"	
"	3	"	6	"	
"	4	"	18	"	

And yet with such discrepancy in the results of these four axles, the deflections measured after each fall of the drop are almost identical. It may be said that the results given above are too few to draw conclusions from, or that the discrepancy in results is caused by bad working or inferior steel. The only answer to which is that they are no more subject to such dangers than any other commercial products that may be furnished, and in fact the writer believes these instances are less so than usual, as they were watched with great care. To me they are a few among a great many illustrations that testing machine samples do not in all cases convey accurate information in regard to the proper quality of material for structural purposes, and that tension tests will not give us reliable data as to resistance of material against shock or impact which is so important a factor in the life of material in service. They also illustrate the folly of accepting finished material upon tests of the same in a preparatory state, which test should be for the guidance of the manufacturer only and not for the engineer. I believe we do give sufficient importance to tests of material within the elastic limit, but place too much dependence upon results obtained from ultimate destruction. The above remarks are offered more as suggestions than as proof of any theory, and I must ask the indulgence of the Society for the hurried manner in which they are thrown together.

Mr. SAMUEL TOBIAS WAGNER, M. Am. Soc. C. E.—While the paper before us is wonderfully rich in statements regarding the high carbon steel, on which such data is very scarce, it is as to certain properties of low carbon structural steels that I wish to make a few remarks. A very simple thing, but one too often overlooked, is the method of preparation of small test pieces both in iron and steel, but especially the latter, and it is a point that inspecting engineers can well afford to investigate.

The point referred to is one specially pointed out by Mr. Metcalf, and is that surfaces should be free from tool marks and should be smooth. The final preparation of test pieces should always be done on a planer, and by a first-class mechanic, and the surfaces and edges examined with care if a fair test is expected. While in the testing machine, the appearance of a crack in a steel specimen is a sure sign of destruction, as we have no fibers as in iron to arrest the tearing.

Regarding the care necessary in heating steel, I would call attention to one paragraph in the Report of the Naval Advisory Board on the

"Mild Steel Used in the Construction of the Cruisers Atalanta, Dolphin Boston and Chicago," prepared by Assistant Naval Constructor R. Gatewood, U. S. N. (1886).

"In making the quenching tests* at Phœnixville the pieces were heated generally in a smith's forge with blast, in a hollow fire, either roofed with wet coke, or with a board laid over the top and covered with pressed coke. Sometimes they were heated in a small furnace burning soft coal. On one occasion about sixty pieces were placed, five or six at a time, on the top of a close anthracite fire under a small vertical shop boiler, being thus slowly heated to exactly the desired temperature and perfectly uniform. All of these pieces failed to bend as required, some of them cracking all across with bright crystalline fracture at the first blow of the hammer, and with no reduction at the fracture, while second pieces from the same bars heated as usual passed the test well. It is regretted that the cause of this behavior could not be exactly defined. It plainly consists in the surface absorption of some brittle making element from the fuel, most probably of sulphur, though it has been proposed that the atmosphere surrounding the pieces being highly carbonaceous (the fire was dull and the pieces were twenty to twenty-five minutes heating), the trouble might arise from a surface cementation or absorption of carbon itself. At all events this extreme case illustrates the well-known necessity of using pure fuel in heating and especially in making expensive flangings, and points to a possible solution of some of the inexplicable fractures of such plates, while it conclusively shows that the circumstances of making this apparently simple hardening test need some attention."

It would perhaps be advisable to add as to the quality of this steel that the average of 133 accepted heats gave

Ultimate tensile strength, 64,020 pounds per square inch.
Average ductility, 25.52 per cent. in 8 inches.
Carbon, .1633.
Manganese, .443.
Phosphorus, .0875.

The theory that iron and steel crystallize under the action of repeated blows and strains, I think is very well replied to by Mr. Metcalf; and while the theory has many staunch and able supporters, I do not feel justified by the facts that have come under my personal notice in thinking that such a crystallizing action takes place, but rather that the result is caused by some inherent defect in the metal, or that some abnormal stress under unwarranted circumstances has produced it.

The fracture of a full sized member of a structure under suspicious circumstances, revealing a fracture partly good, and a spot or one side containing large, bright crystals, I do not consider should always be charged against the material, nor do I believe that any but an expert

*Quenching Test. "A test piece shall be cut from each angle, plate or beam, and after heating to a cherry red, plunged in water at a temperature of 80 degrees Fahr. Thus prepared, it must be possible to bend the pieces under a press or hammer, so that they shall be doubled around a curve of which the diameter is not more than one-and-one-half times the thickness of the plates tested, without presenting any trace of cracking."

can safely say whether it has been over-heated, or whether it is due to a sudden break under high strain.

A full sized iron eye-bar, 5 x 1½ inches, was tested, and upon reaching the ultimate strength gradually began tearing through a slight flaw on the under side as it lay in the machine. After two-thirds of the section had torn, the bar broke suddenly across the remaining third, revealing a section one-third of which was completely crystalline and the remainder beautifully fibrous. The bar was nicked with a cold chisel three inches from the fracture and broken under a hydraulic press, revealing a fracture entirely fibrous. One not an expert, would, upon seeing the original fracture, have been justified in pronouncing the bar over-heated on one side.

Mr. D. J. WHITTEMORE, Past President Am. Soc. C. E.—My own experience is that as a rule steel suffers about ten per cent. in strength value after it leaves the mill and has gone through the necessary shop manipulation to transform it into bars for bridge purposes. While it appears homogeneous before being wrought, this characteristic is, in a measure, lost on leaving the shop, and now the question is: How can this be prevented? I know that in the manufacture of eye-bars it is claimed that iron suffers the same relative deterioration, and I also know that nearly every manufacturer will claim that this is not so by their processes of manufacture.

I thank you heartily for giving this paper to our Society, and trust that those of our Members who feel competent to treat this subject with some degree of intelligence will give it that attention that is so meritoriously deserves.

In general, it may with truth be said that many of our engineers are as ignorant of this subject as we were when Scott Russell drew marked attention to and warning of its peculiarities thirty years or more ago.

Mr. JOSEPH M. WILSON, M. Am. Soc. C. E.—Mr. Metcalf gives us much information of value in reference to steel, and in a manner that makes it exceedingly interesting. Surely, with all the knowledge in reference to the manufacture and management of steel which this article intimates, and with the ability of the manufacturer to control the quality produced, which he evidently possesses, the engineer ought to be able to obtain in this material what he wants for his particular purposes. Also the knowledge of the effects produced by conditions of heating, etc., should warn him to exercise that care so necessary in the manipulation of this material, as he shapes it to the forms he desires.

I quite agree with Mr. Metcalf in his remarks on the crystallization theory. I have had wrought-iron tested that had been some twenty-five or thirty years under heavy service, where I knew it had been considerably overstrained, and yet the results were so excellent as to leave no reasonable doubt but that it was still in as good condition as when first

put in use. I have also known brass-wire chains, by which hanging baskets of flowers were suspended, and which, when new, were amply strong for their purposes, to become so rotten from the action of the air as to break under their load, the whole character of the wire having been changed undoubtedly by a chemical action of the air, not from crystallization under service.

I do not profess to be an authority on the manufacture of guns, but Mr. Metcalf's remarks strike me as full of common sense and quite *apropos* to the occasion.

I would like to ask Mr. Metcalf if he knows of any way of testing for over-annealing in eye-bars without injury to the bar?

Mr. WILLIAM METCALF.—I do not know of any way to test an eye-bar for over-annealing without injury to the bar, unless it would be by discovering a greatly reduced elastic limit. I have no data on the point, but from the general worthlessness of badly over-annealed steel, I would "guess" that its elastic limit would be affected seriously.

Mr. L. L. BUCK, M. Am. Soc. C. E.—Steel possesses so many desirable qualities, such as its great strength, its susceptibility of receiving a fine, true finish, its resistance to corrosion, on the one hand, while, on the other hand, it is so difficult to ascertain what serious defects may lie hidden within any piece from which satisfactory test pieces have been cut, that a paper on the subject by a gentleman who had been so long engaged in the manufacture as has Mr. Metcalf must always possess a peculiar value.

That part of the paper relating to comparative tests of steel in the cast state with that which has been subjected to the process of rolling or hammering, suggests a direction in which investigation might be pursued to advantage in selecting steel for structural purposes.

Most of our tests have been made upon specimens prepared from each blow by rolling or hammering, and upon the results of such tests we base an acceptance or rejection of the whole blow. Consequently we sometimes find, out of a number of bars or plates rolled from an accepted "blow," one that exhibits characteristics that, appearing in the test piece, would have caused the proper rejection of the whole blow. If then, in addition to prepared test pieces, we were to have a cast test piece made in each case, we should not only learn whether such defect as appeared in the tests were due to the blow itself or to subsequent manipulation, but we would soon have data of great value as to the actual strength of steel castings.

In all the discussions upon the subject of heavy guns that have come under my notice, the effect of the inertia of the mass of the gun in resisting the almost instantaneous impact of the discharge has been either overlooked or neglected.

If we suppose a very thick and perfectly homogeneous hollow cylin-

der of any metal, and with no initial stress in any part of it, and that a heavy bursting stress be applied to it gradually, there will result a greater increment of the inner than the outer diameter, due not alone to the fact that a large cylinder having a sectional area equal to that of a smaller one must be thinner, but also that there is a radial outward compression of the material, as well as circumferential tension. On this account the tensive stress at the surface of the bore is rendered still greater than would otherwise be the case. If now, instead of applying the pressure slowly, it should be produced by an explosion of gunpowder, the same radial compression takes place, but very much greater at that portion next to the inner surface, while it is less toward the exterior, and, consequently, the impact of the discharge is resisted by the inertia of but a small portion of the surrounding mass. In such a case very little of the stress produced would ever reach the exterior, as before it could do so the inner surface would crack, whereby the area pressed upon would be increased with comparatively slight increase of volume of gas, and the cylinder would merely tear apart.

If we can produce a cylinder having a strong initial circumferential tension in the outer surface, and gradually decreasing to the interior till it produces a compression within, and that greatest at the surface of the bore, we shall not only have the strength of the section, but the inertia of the mass as well, to resist the impact.

As to the forging of large masses of steel (or iron), there appears to be no way of accomplishing it which may not be termed "barbarous." Whether rolled, hammered, or compressed, such working must fail to extend to the interior, unless the piece is hollow, and the work and cooling proceed inside as well as without. Otherwise there will always be a portion of the interior that will not only not receive a proper amount of work, but, being the last to cool, will remain with a strong initial tension. This will tend to render whatever defects it originally presented—as, for instance, piping—still more serious. At the same time that the interior surface is subjected to tension, there must necessarily be a compression of the outer circumference. Such a condition is manifestly the worst that could exist where the cylinder is a gun, and one which experimenters have endeavored to reverse, but, as the paper states, with only partial success, if we except the case of the Rodman cast-iron gun.

A process may be found in the future whereby a steel gun of large size can be forged on the inside while the outside is made to be the last to cool, and thus produce the proper conditions. Even if the separate bands of a gun could be made in this way, the built gun would be a successful but costly one. But as at present forged, probably but a small portion of total strength of the section of a solid forged gun is developed by an explosion. The same condition exists in each of the thick bands of the built gun, though to a less degree.

The practicability of making a steel cast gun successfully must depend upon whether a steel cast gun of such massiveness as is required for

a large gun can be produced sound and strong enough to give it the requisite strength per square inch of section. If so, there appears to be no insurmountable obstacle to produce a successful gun in the manner proposed by Mr. Metcalf, as, without doubt, the other requisite conditions could be acquired as well with the steel castings as with iron castings. The unit strength of the metal could be considerably less in the cast than in the forged gun, and yet make a stronger gun.

Of course any slight cavity in the inner surface of the steel cast gun would be inadmissible. But if on boring the gun a slight cavity should appear, the gun could be bored larger, and have a forged steel brush turned and forced into it. Indeed it is possible that the steel cast gun might be made a success, if a breech-loader, by casting it on the Rodman principle, then bore it with a very slight taper (slightly largest at the breech); then make a forged cast-steel tube of a pretty high grade of steel, and as thin as might be found practicable, turn it to exactly the same taper as the bored cavity, as well as of the proper size; warm the casting, and let the tube be compressed by the cooling. The tube would then receive a high finish, and present a durable, strong surface to resist the action of the powder and projectile.

Even supposing but one casting out of several should prove perfect enough to sanction its use, we should still have an economical gun.

If such a casting should have a large cavity, or a number of them, which did not appear in any part of the surface, they would be detected by weighing, or by the final tests at the "proving grounds."

GUSTAV LINDENTHAL, M. Am. Soc. C. E.—The subject of Mr. Metcalf's admirable paper on steel and its properties, suggests so many thoughts and investigations, that to discuss even a small part of them would make a lengthy paper. I wish only to make a few remarks, therefore, in relation to structural steel.

The statement that iron and steel are liquids, seems to be more in the way of a paradox than a definition. And even the paradox would be obscure without Professor Langley's elucidation, which is meant to apply to other metals also. This theory has a truly mathematical aspect in starting out with a general law, the application of which to metals is at once fascinating and promising of great results in the understanding of their nature; but, as in mathematics, a certain fixed nomenclature of terms and definitions will be necessary before the inductive process of reasoning can be carried very far. At present there is some confusion as to the meaning of vibration, impact, oscillation, concussion, crystallization, etc., and exact reasoning is thereby much impaired. As a consequence, some notions, amounting almost to axioms, have been ingrained into our minds in school and in practice of which we have to rid ourselves first before we can look at things without bias; as, for instance, that rails in a track and car axles will change their texture in consequence

of vibration and become crystalline (as it is called), from which long ago has been argued by eminent authorities that iron and steel bridges in course of time must fail from like cause.

But recently again the cold of winter has been accused of changing iron and steel from a fibrous or ductile condition to one of coarse crystalline texture, and an array of tests was furnished in evidence. And as another instance, Mr. Metcalf had at one time furnished facts and his experience with steam-hammer rods and connection bars of locomotives, from which was deduced the theory that hard steel is much better than soft steel for resisting vibrations, which Mr. Metcalf assumes differs from alternate strains only in degree.

It seems to me that the true explanation of failures in above and similar instances is from other causes.

We know of nothing so injurious to the physical structure of iron and steel as the sudden reversion of strains, and next to it sudden variation of same kind of strains. The metal requires a certain time to absorb the strain and to accommodate itself to it. If this time is not given, then a gradual weakening and ultimate failure is the result.

We know from many experiments that the more ductile the metal is the better can it resist sudden variations of strains.

Ductility expresses but the greater capacity of the metal to flow under the application of force; or the more liquid it is (to make use of the new theory) the better it will resist fracture.

In point of liquidity we have soft wrought-iron first, then the low steels, and least liquid are the high steels. Just in this order they fail under tests, namely, wrought-iron gives the greatest number of reversions, and high steel the least for the same ratio of strains to ultimate limit.

It is natural for a steel-man like Mr. Metcalf to say that "mild steel, which is commonly used for structural purposes, is more ductile, stronger, and tougher than iron;" he should have added, in the testing machine. Mr. Benjamin Baker in his paper read before the American Society of Mechanical Engineers in 1886* presented tabulated results of tests with spindles of wrought-iron, soft and hard steel, subjected to reversion of strains. These tests are important and instructive, but in a somewhat different way than the one assumed by Mr. Baker, for he is committed to steel, and his paper is presented "to bear testimony to the admirable behavior of a very good friend of his, namely, mild steel." Nevertheless his very tests show that wrought-iron gave the best results, but he does not make as much of them as those with soft steel.

Thus his table (series No. 1) shows for soft steel from 68 400 to 155-295 reversions; hard steel, from 5 700 to 7 500; best bar iron, 389 050 to

* Some Notes on the Working Stress of Iron and Steel: Benjamin Baker. Trans. Am. Soc. Mech. Engrs. Vol. VIII. 1887. Page 157.

421 470 reversions for practically the same ratio of strain to ultimate limit (factor a of this table), namely, 1.84 to 1.90.

In my paper on the Monongahela Bridge,* I took occasion to state the experience had in the old Suspension Bridge with short suspension rods subject to alternate bending strains, and I mentioned the fact that good soft wrought-iron rods would last four to six times as long as good selected soft steel, which agrees very well with the tests made by Mr. Baker.

His reference to a greater number of mysterious fractures in a few tons of wrought-iron, presumably of indifferent quality and for temporary purposes, than in 24 000 tons of mild steel, is surprising, but too general for serious consideration. Ordinary English wrought-iron must be of poor quality indeed if a good quality costs double and triple the price of Mr. Baker's steel. It is not so in this country, where excellent bridge-iron is still cheaper than the lowest-priced structural steel.

Rails in the track and car-axles are subject to rapid reversions of strains, which ultimately destroy the cohesiveness of the metal and cause failure, but not necessarily so. For if the rails and axles are large enough, so that the reversions of strains are kept far within the elastic limit of the material, they will last very much longer. The term crystallization in reference to such fractures is used with too much laxity; granulation would be a better term. There are no crystals in the fracture, and no crystals can form out of rigid and cold iron or steel. The fine-grained texture simply changes to a coarse-grained texture, or the fine grains ball up to coarse grains by reason of the inability of the iron to flow fast enough under sudden strains or under a reversion of strains.

Vibration has nothing to do with it. Vibration is the result of impact or sudden strain. It is a wave-like, exceedingly rapid motion of the metal far within the elastic limit, and tending towards rest, as in a piano-string. Impact can cause fracture, but vibration does not.

Oscillation is something different. It is the result of intermittent strains of same kind produced by variations in the action of loads or forces. Oscillation is cumulative, and tends towards fracture without a reversion of strain. A bridge under the regular step of soldiers oscillates, and if sufficiently long continued would break the bridge down. Impact, intermittent strains and strains from steady load can happen in a structure all at the same time. Their different effects then become superimposed.

The breaking of steam-hammer rods and connecting rods of locomotives is not due to vibration (or even oscillation) as alleged, but to the sudden reversions of strain. Mr. Metcalf's version has been cited frequently, but I believe it to be erroneous.

---

* Rebuilding of the Monongahela Bridge, at Pittsburgh, Pa. G. Lindenthal, M. Am. Soc. C. E. Trans. Am. Soc. C. E., Vol. XII, p. 353. September, 1883.

His experience was that wrought-iron and mild steel hammer rods would frequently break. By accident a high steel rod was taken at one time to replace a broken one of mild steel, and it lasted much longer than any before. Mr. Metcalf ascribed this to the greater rigidity of high steel in resisting vibration. The fact is, however, that the high steel rod was simply so much stronger. The strains which the rod had to resist were, we will say for illustration, 10 000 pounds per square inch in soft steel, with an ultimate strength of 50 000 to 70 000 pounds. Taking now a material however of 100 000 pounds ultimate resistance, and keeping the diameter of the rod unchanged, must result in greater durability. Had the high steel rod been reduced in size to correspond with the same ratio of strains as in mild steel, the high steel would have been broken sooner than any of the others.

The same reasoning applies to connection rods of locomotives. Of such rods, one of wrought-iron, one of mild steel, and one of high steel, and all of good quality, the rod of high steel must necessarily last longest. But make these rods of sections, so that each would have the same ratio of unit strain to ultimate strain, then the high steel would break first, the mild steel next, and the wrought iron last.

This would be in agreement with the views of liquidity of iron and steel as stated by Professor Langley, and this is confirmed by Mr. Benjamin Baker's experiments and by my own experience with iron from old bridges.

The lesson to bridge engineers from this is, that in steel bridges the ratio of unit strain to ultimate limit; or, better yet, to elastic limit, should be greater than in wrought-iron bridges for the same margin of safety. But this rule should be used with discretion. It should be kept in view that for a quiescent load the conditions of durability are the most favorable, and that they are the same in all three metals. That the durability (or safety) is least in members exposed to reversion of strains or to suddenly applied strains, and differing in the three metals; and that between those two limits of durability or safety in each metal we should graduate the unit strains in accordance with the changeability of the total strains which the member has to resist.

The bulkiest bridge is the best. As wrought-iron gives a more bulky bridge than one of steel, it is the best for all ordinary spans. Practicing engineers have, from their experience, been led to this view for some time. In longer spans other questions arise, which may modify somewhat the former conclusions. The relation of dead to live load is growing more favorable as the spans become longer, and also the conditions for certain members with cumulative strains become more favorable, so that high steel may be used for such members without loss of safety.

But again. Of three bridges, one of wrought-iron, one of mild steel, and one of high steel, and for same length of span, all of the same bulk, the high steel bridge will be the strongest and most durable.

On the other hand, where the type of construction is such as to produce large secondary strains, as in riveted structures, a high steel should not be used, because it is wanting in sufficient ductility, or liquidity, to resist them without injury. Then there are the textural difference in wrought-iron and steel to be considered in the choice for structural material.

Steel is crystalline in texture; wrought-iron is fibrous. I am aware of the theory which assumes that wrought-iron is also crystalline, only that the crystals are drawn out in the rolls or under the hammer between fine layers of slag. This theory is more elegant than true, and useful only to dialecticians defending the universal crystal theory. We know that wrought-iron owes its great virtues to its fibrous texture. Good wrought-iron varies but little in ultimate limit and elastic limit; eight per cent. for the first and five per cent. for the latter will cover the range of differences. Wrought-iron is not only ductile and tough, but it is also hardy. Scratches, nicks, dents, rough handling, do not hurt it much. During shop manipulation and in the blacksmith's fire it is not apt to get hurt much if of good quality.

But steel, soft or hard, is a very much more sensitive material, and the greatest care has to be observed in its manipulation. To make sure that it did not get hurt in the shop it afterwards requires doctoring in an annealing furnace, and often this doctoring makes it worse, as Mr. Metcalf himself states in his paper.

The variations in ultimate and elastic limits are greater in steel than in wrought-iron. For the same quality they vary so much as sixteen per cent. for ultimate and ten per cent. for elastic limit, or double as much as in wrought-iron.

If the conditions to be observed in the working with wrought-iron are half a dozen, then for soft steel they are at least twice as many, and for hard steel three times as many. If after all this there results a certain economy in the use of good steel under safe conditions in a bridge, it should be used, otherwise it should not be used. Steel for all bridges, for all spans, and steel as the metal of the near or distant future for everything, seems to me a bad rule. And in this connection I should like to ask of steel manufacturers the question: What kind of steel can that be which is sometimes bought for bridges at prices nearly as low as for ordinary wrought-iron in the present state of the art?

To make good bridge steel costs more than a corresponding good wrought-iron in this country. And if a doctored cast and rolled product resembling steel stands certain meager tests in the testing machine, it is not a sufficient warrant for putting it into a bridge, with dimensions so attenuated, as if it were of most excellent quality. The time will come when "true as steel" will not be true of such bridges.

On the alleged influence of frost on iron and steel, the tests of Mr. Joseph Ramsey, Jr., Chief Engineer Cincinnati, Hamilton and Dayton

R. R., have been mentioned as giving some new light on the question. I doubt somewhat the value of the tests as given in his paper before the Engineers' Society of Western Pennsylvania.* A specimen taken from a freezing mixture to a testing machine will not give accurate results. The surface warms quicker than the interior of the specimen, which therefore has inert strains; the skin expands faster than the interior of the specimen. In this condition a blow from a drop weight strikes it and a short fracture is the result. From thousands of tests, made in the cold of winter, the uniform lesson was that no difference in strength or texture existed in the iron in the extreme temperature of our climate. If the woodman warms his axe in the winter before using it, are we not rather to assume as a reason that by striking the cold material into the hard frozen crust of a tree, heat is generated which will expand the thin edge much faster than the bulky metal back of it, and therefore will crack it off?

The greater frequency of broken rails in the winter can be accounted for in a similar manner.

First.—There is the well-known explanation of the unyieldingness of frozen ballast, exposing the rail to anvil-like blows.

Second.—There is the fact that the under side of the rail in contact with the frozen ground is cold and contracted, while the top and side on a sunny winter day are exposed to the heating rays of the sun.

Uneven expansion and inert strains are set up; the rail, if it could, would like to curve up in the middle. Now comes the train and delivers blow upon blow on a rail weakened by inert strains; and fractures at the weakest spots, such as are experienced, are the consequence. Most of the so-called mysteries could perhaps be explained in a similar way.

Mr. Metcalf states that "a great railroad company discovered that the moduli of elasticity of mild, medium and hard steels, tempered and untempered, were practically the same."

This is important, if true, and it would have been well to mention more of the facts in the case and to give the authority of the experimenter. For if the facts are as stated, it is against the new theory of liquidity of iron and steel in which Mr. Metcalf otherwise firmly believes. He further says it was proven by the fact that coiled springs of any kind of steel, if bars were made of the proper size according to a formula based on same moduli of elasticity, were all right. Now this may be true and still prove nothing of the kind.

I doubt the correctness of the conclusions as to the same modulus of elasticity in all kinds of steel, and in the absence of detailed facts I should prefer sticking to the old view of proven great variations in the moduli. It is possible that Mr. Metcalf mistook some other relation, of which the modulus of elasticity was a function, for the latter alone.

---

*The Effect of Temperature upon Structural Iron and Steel: Joseph Ramsey, Proc. Engrs. Soc. Western Penna. Oct. 18, 1887.

The use of steel for heavy guns is a topic which received more consideration perhaps than any other in the discussion of Mr. Metcalf's paper. In view of the strains the gun has to resist, the metal should have a high elastic limit and great ductility combined. This we find only in the very best of tool steels and in small specimens. Mr. Metcalf contends that the necessary physical conditions in gun steel can be produced through the agency of heat and cooling, as by casting on the Rodman principle. But whether cast in this way, or forged by the Krupp method or the Whitworth process, what confidence can we have in the use of steel in large masses, when tests will show that it will break in bulk with sometimes as low as 39 000 pounds per square inch, when the same steel in small specimens in the testing machine will show 100 000 pounds per square inch?

Wrought-iron does not show such great range of uncertainty, but so far has not otherwise been adapted for the production of large cannon.

Would not the so-called Mitis process of casting wrought-iron offer a solution of these difficulties? Supposing a tube of fine hard steel (to resist the abrasion from the shot or projectile) as a core, around which were cast by the Mitis process the body of the gun of wrought-iron; it would melt the outside of the hard steel tube, become thoroughly welded to it and form one whole piece. The ferro-aluminium used in the Mitis process as an addition to wrought-iron is claimed to also prevent the crystallization of the cooling metal, which in addition has greater strength than wrought-iron alone. It seems to me in that direction there is yet a field for great developments.

Mr. WILLIAM METCALF.—I made no mistake about the modulus of elasticity; the data can be found in Mr. Cloud's paper on springs, read before the American Society of Mechanical Engineers at its Pittsburg meeting.*

If Mr. Lindenthal had read more carefully, he would have observed that I did not say springs of iron, mild, medium and hard steel, tempered and untempered, would be equally good, but that the formula said so; but the railroad referred to adopted unusually high steel, tempered, in spite of the formula.

Mr. THEODORE COOPER, M. Am. Soc. C. E.—The valuable and instructive paper just read is one of especial interest to us. We all realize that steel is to be the structural material of the future; and with a material covering such a wide range of properties, it is absolutely necessary that we should know its characteristics and capabilities. In order to take full advantage of these properties, and thus obtain the best results, we must be able to select which particular grade of this material is best adapted to our purposes. To do so we must know not solely the "test-

* Helical Springs: John W. Cloud. Trans. Am. Soc. Mech. Eng., Vol. V, 1884, p. 173.

ing machine" characteristics of the material, but in what manner each particular grade of the material will be affected by the processes of manufacture through which it must pass before it forms our final structures.

Papers like the one before us, from men who are daily watching and studying its characteristics; noting defective results and searching for the reasons thereof; not proving their conditions by simple laboratory tests, but by the successful attempt of a manufacturing establishment of no mean character, demand our careful study and consideration.

Considering the increased sensitiveness of the higher grades of steel to the action of heat and work, through one or both of which all our structural material must pass during the processes of manufacture, it is our duty to give this fact due weight before selecting the higher grades solely from the increased tensile strength expected.

While I believe we should give preference to the soft or mild steels for such structures as boilers, ships and bridges, I should adopt high steel for guns.

Where we must have the highest resisting power with a minimum weight of material, we must use high steel, and are justified in the additional care and expense by the necessities of the case. In guns we want a high elastic limit and have need of only a moderate ductility. I believe a steel-cast gun can be made which can compete successfully with any form of built-up guns; but I fully realize that there is a costly experimental stage before success is reached. Having faith in our ability to improve upon the best European practice, I would like to see our manufacturers given a fair chance. They should not be expected to enter upon the expense and study without a proper reward in expectation.

Let the Government say to the manufacturers of this country: "If you will give us a certain number of guns capable of standing certain requirements, fully equal to the best guns now made, when compared with due regard to cost and weight, we will order so many guns from you."

Our manufacturers will then have a proper incentive to prepare for such work. The Government can thus get the advantage of the fullest and freest competition without detriment to the results.

I believe Congress has appropriated $24 000 for making three cast guns, but certainly this was not intended in good faith, for who could afford to make the preliminary expenditures and investigations and test the guns for such a sum alone, without any positive promise to keep the plant in reasonable operation upon the guns fulfilling all the requirements?

I do not believe the metal in a built-up gun can be put in that perfect condition which theory requires in order to get the expected result.

In my opinion there is something more to be taken into the account

than solely the hoop tension. I cannot believe that a series of concentric rings will resist the radical forces of expansion as well as the same amount of solid metal.

On expanding a circular ring of metal, the metal must flow on certain geometric curves similar to the flow lines produced by punching a piece of steel. These lines, as shown by me in a paper read before the Society,* will be approximately at 45 degrees to the lines of the applied force, which in this case will be radical.

These flow lines measure the resisting power of the material, and being continuous in the solid gun, would lead me to expect a greater resisting power than with the same material in successive rings. This is more a matter of faith than proof, I acknowledge.

Mr. EMERY—If I take two bars, 50 diameters in length, or a single bar 50 diameters in length, the single bar having exactly the same quality as the two bars, and break them purely with tension, which will break the easiest, the one or the two?

Mr. COOPER—The one.

Mr. EMERY—My own belief is that as regards the two and the one the strength is identical.

Mr. COOPER—No, because in the smaller bar the metal flows more uniformly than it will in the large bar, and hence the average strength of all the fibers will be greater.

Mr. EMERY—They should be of the same quality.

WILLIAM METCALF, M. Am. Soc. C. E.—I close the discussion with a feeling of disappointment that the subject of steel has been so largely overshadowed by the gun question.

In the latter I have no interest beyond that of any citizen, and if I had known that our Ordnance Departments had advanced so far, and had decided upon their plans so fully, it is more than probable that I would not have said a word on the subject publicly. But when Ordnance Commissions and Special Committees of Congress were traveling over the country seeking information, and were scattering their circulars of questions broadcast, was it not natural to assume that some one wanted to know something?

When, too, these same commissions and committees buried their work and their information in Government volumes not in general circulation, and allowed the daily press, uncontradicted, to assert for them over and over again that neither steel nor guns could be made in America at all comparable to what could be bought in Europe, was it not natural for a full-blooded Yankee to rise in wrath and attack them?

Mr. Dorsey then, first, and myself, later, have done a good work in drawing the officers out of their shells. They have proven themselves

*Observations on the Stresses developed in Metallic Bars by Applied Forces. By Theodore Cooper, M. Am. Soc. C. E. Transactions, Vol. VII, p. 174, July 1878.

to be hard-working, painstaking, able gentlemen, genial, courteous, and well fitted to maintain their position.

They have shown us that they have designed and made the best moderate sized steel guns in the world, and they have expressed their satisfaction with American steel-makers.

That is enough; all hail to them! From this time out I shall shout: Give our Ordnance Corps all the money they need, and I shall stick quietly to my opinion that the best way to make a BIG gun is to cast it and treat it by heat alone.

Returning to the subject of steel, Mr. Coffin's contribution is the work clearly of a master mind, and the study of it has given me much pleasure and information.

He first informs us that guns are not annealed after they are hardened in oil, but that they are tempered.

That is just what I supposed, and my criticism of the specifications stands good. It cannot be necessary to remind Mr. Coffin that the words "annealed" and "tempered" are technical and convey exact meanings; he certainly would never order a piece of steel to be annealed when he wanted it tempered.

Mr. Coffin's discussion of Brinnell's conclusions and of his own admirable experiments is almost too technical for a general reader, yet it seems to call for some answer.

Assuming them to be correct, we have the fact that carbon exists in two forms in steel, the "cement" and the "hardening" form; that at a fixed temperature for all grades of steel the change occurs; below that temperature it is "cement," above that temperature it is "hardening."

This theory seems to involve too the other, that in hardened steel we have a definite carbide of iron, and in non-hardened steel the carbide does not exist, although I believe that Mr. Coffin does not make this statement himself.

Mr. Coffin shows farther, by his acid tests, that there are ten forms or conditions of carbon in steel; truly this is a remarkable element, and engineers may well stand aghast at the prospect.

I will not say that there are not cement, hardening, and ten other forms of carbon in steel, for I do not know the contrary. I will believe in these twelve forms when I see them separated and made palpable.

I will not say that there is no definite carbide of iron, but if there be, then by the law of combinations we ought to be able to convert any piece of iron into complete carbide containing something like seven or eight per cent. of carbon; but we cannot put anything like that amount of carbon into iron unless we force it in by the use of manganese.

If there is a carbide of iron, why is it only a little bit in a great matrix of iron?

If a little of the iron will form carbide, why will it not all unite with carbon?

I will ask engineers to set against these remarkable theories Professor Langley's theory, in which I am a firm believer.

Carbon is dissolved in iron; a saturated solution seems to be something under three per cent. of carbon when there is no other element like manganese present to increase the power of solution.

It is simple enough to be at least credible.

Next I offer the theory that hardening is caused by tension; that tempering and annealing are reductions of the tension by heat.

If hardening is a mere change of condition of carbon, why should a piece ever crack when hardened? Let any person take a round bar of steel of any carbon above .60 and overheat it just a little too hot, and with perfect uniformity of temperature, and quench it, and it will split up the middle. A bar of any section will do the same, but the round section splits the easiest. It is no trouble to split a round bar; it is more difficult not to do it. Heat relieves the tension and softens the steel; a given temperature relieves a certain amount of tension and no more; therefore, as Mr. Coffin says truly, if the first annealing does not give you softness enough, no number of repeated heatings to the same temperature will give any greater softness.

It is a happy fact. Given, say a car-spring, it has a certain elasticity, temper, given to it by quenching. It may be frozen to 40 degrees below zero, and heated to 120 or 130 degrees in the sun, thousands upon thousands of times, yet it retains its tension and carries its load for many years. Consider too the permanency and exactness of the springs of watches and clocks. Of course this fact does not militate against the "cement" and "hardening carbon" theory, nor do I understand Mr. Coffin to accept this theory without reserve.

But, under the dual carbon theory, why does a piece of hardened steel lose any of its hardness until the change to "cement" occurs? We all know that hardened steel does grow softer for every single degree of heat to which it is subjected; we can understand this if we accept the tension theory; but it does not seem so plain with the other theory. Professor Langley proved conclusively that a hardening effect was produced when he boiled steel in water, 212 degrees, and quenched it in water at 60 degrees, and he gives us the measure of the hardening effect; surely these temperatures are away inside of the "cement" carbon limit which Mr. Coffin states to be between dark-red and red, in the dark.

Experience teaches that in tool steel at least, we know of no temperature above that of the atmosphere at the time, from which, if steel be suddenly quenched it will not show that it is hardened to an amount due to the temperature applied.

We cannot recognize any fixed temperature where hardening begins. On the contrary, it is a function of the temperature and has some value for every degree of temperature. Also there is a regular increase of

softness for every degree of heat that is added, until steel becomes liquid.

Mr. Coffin's explanation of Mr. Brinnell's chart is interesting, and would be equally instructive to engineers if only their eyes were trained to the study of fractures, so that they could see mentally the structures meant by "fine crystalline," "flaky crystalline," "coarse crystalline."

The results given, as far as they can be explained in words, agree with our shop experience, and they indicate clearly what has long been an axiom with us, *i. e.*, that a fracture of steel always indicates the highest temperature to which the steel was last subjected, no matter how it may have been cooled, provided it had not been hammered or rolled, or otherwise worked mechanically.

This fact is of inestimable value to a steel-maker, and it would be of great value to engineers if they would study it.

I would not know how to try to describe the fractures intelligibly without samples, but the mode of learning is simple. Go to the scrap heap; select a lot of pieces (if of known carbon so much the better) and heat them to various temperatures and in varying times, and cool them in different ways. Then break the pieces and study the fractures, singly and in groups.

This costs nothing and is a good amusement for leisure hours. I have known men to become complete enthusiasts over old scrap heaps. There is plenty of scrap about every establishment. One of the most interesting to observe is the fact that all steel, from "dead soft" to the highest carbon, will go through similar changes of structure.

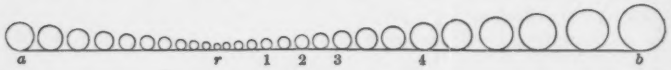
The fractures are not alike for the various carbons, but they are so similar that they will soon convince any one that they all obey the same law.

It is not to be supposed when we say that steel always records its highest temperature, no matter how it may be cooled, that a bar quenched, and another cooled in air, and a third cooled in a bed of warm ashes, from the same temperature, will have the same fractures; they will be markedly different. But a thousand bars, or a million bars, of the same composition, heated to the same degree, and cooled in the same way, will have identical fractures; so that the lesson learned once is learned for all time.

Mr. Coffin says all steels refine at the same temperature. This is so entirely contrary to our experience, that I cannot believe Mr. Coffin can have examined the matter for himself. We recognize a distinctly different temperature for refining for at least fifteen different tempers of steel, and we act upon this knowledge every day. The range is not great, but it is palpable. In steels of great difference of carbon the range is considerable; for instance, the temperature at which .30 carbon steel will refine will almost certainly break a piece of 1.50 carbon steel every time it is applied.

Perhaps it will be better to explain this refining fully.

Take any ordinary bar of steel and break it and let the size of the grain be represented by a circle, the first one in the row below; then heat the bar to different degrees and quench it from each; in this way for every change of temperature that is visible to the eye there will be a change in the size of the grain or in the texture that is equally visible to the eye. If the heats range from the lowest red, or possibly below red, to the highest attainable, say scintillating, and the corners melting, our row of circles representing coarseness of texture will be like this



b being more or less greater than a, owing to the condition in which the workman left the bar, fine or coarse.

b will always scratch glass if the steel is high, and it will always be about as brittle as glass.

There are some interesting facts connected with that row of circles.

Perhaps the most interesting, because it is the most surprising, is the fact that the specific gravities decrease regularly from a to b. It would seem as if they ought to increase from a to r and then to decrease from r to b, but they don't; they decrease from a to b and much more rapidly and to a much greater amount in high carbon than in low carbon steels. This is the "mystery" of cracking and brittleness in high steel—the change of volume is greater and the tension is greater.

How or why r is of greater volume than its neighbors to the left, which have a coarser texture than r, is an unaccountable mystery to me.

At one time I seized upon the "cement" carbon and "hardening" carbon, and the "critical" temperature theories for an explanation, but they will not do, first, because all of the circles from a to r are harder than a, and each is harder than its neighbor to the left; therefore r cannot be the critical temperature where carbon changes form, because the circles below r are hardened.

Second.—Because r has no uniform position in the row, it will shift towards a in high carbon steels and towards b in low carbon steels.

Another interesting fact about the circles is that if you take a piece made up of circles b and heat it to the temperature of circles 4 and quench it, the fracture will be circles 4; if you heat it to 3 you will get circles 3; 2 will give circles 2; and r will give circles r, and so on.

This is called "restoring" steel; and it requires no nostrums but fire and water. Now to apply the row of circles to engineers' uses.

First.—Experience teaches that r is the best condition to get any steel into. But as r moves away to the right in low steels (structural steels) it is obvious that mild steels will bear a high temperature without

"raising the grain," therefore there is not much danger of injuring mild steel by moderately high heat if ordinary care be used.

But some care must be had, for any steel can be ruined.

Second.—The "restoring" property makes it possible to anneal even circle *b* back to circle *r*. This shows the value and importance of good annealing. But I do not advise any one to make a practice of getting *b* circles in steel except experimentally; the operation is one of disintegration and it injures steel permanently.

Third—And possibly the most important of all, is that the difference in specific gravity (the difference in volume as represented by specific gravity between *a* and *b*) is in low steels much less than in high steels; as for instance, for 40-carbon the difference between *a* and *b* is only one-fifth of the difference shown in 120-carbon steel.

This is the reason that mild steel is not easily ruptured by a high heat; it is also the reason that a mild steel may be rehardened many more times than a high steel without breaking. It shows too, if the tension theory be correct, that it is difficult to set up a dangerous strain in mild steel, so that engineers need have little fear about using it.

Steel that has been heated once to *b* may be restored to *r*, and if it is high steel it will be hard and will hold a fairly good edge; but steel that has been heated to only an orange color and is left soaking in the fire for hours until it is thoroughly over-annealed, will not harden properly. If it hardens at all it will not temper true, and it will be crumbly or soft and will not hold an edge.

I have known "dead soft" steel to be over-annealed so that no amount of subsequent annealing and coaxing would make it anything but rotten and worthless. I believe this comes more from soaking heat than quick over-heating. I thought at one time that it was caused by a permanent change in the condition of the carbon in the steel; that it became like an exceedingly fine cast-iron; but this will not hold, because cast-iron will harden and temper if it does not contain too much silicon; neither does it accord with either the "cement and hardening" carbon theory, or the tension theory. Therefore, as I said before, I suspect that the change is effected by the absorption of some gas.

To anneal properly, then, heat to the lowest heat that will give the degree of softness required and allow a short time for the particles to arrange themselves; then cool as slowly as possible, the slower the cooling the greater the softness that is retained. Heat produces the softness, slow cooling retains it.

I have stated that all steel will harden if it contains any carbon at all, and also that even cast-iron will harden and anneal; this statement includes the so-called "self-hardening" steels.

It is easy to anneal the hardest self hardening steel so that it may be drilled, filed, and cut, like any other steel, and then to harden it. The best quenching medium for "self-hardened" steel is air; water or oil will crack it almost every time.

One more point. Mr. A. M. Howe stated recently, on the authority of Bessemer and Chernoff, that the size of grain depended on the rate of cooling. That slow cooling always produced coarse crystals, and quick cooling produced fine crystals. Their experiments bear them out as far as they went.

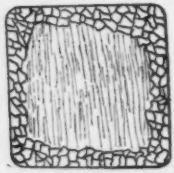
It is hazardous to differ from such eminent authorities; but I do differ from them, because experience shows the general statement to be misleading. It requires a mass of steel to show their experiments; and then it shows another thing, it shows that the difference is due to the tension caused by quick cooling, and that it will not hold in small sections.

For instance, if a piece of steel an eighth of an inch thick, or even a quarter of an inch thick, be heated to its temperature *r*, it matters little how it is cooled, the grain will be fine, and the difference between a quenched piece and a slowly cooled piece will be hardly appreciable.

If a ½-inch piece be heated to *a b* circle condition, and one end be quenched and the other end be allowed to cool slowly, the quenched end will have rather the larger grain.

It is by this that we detect the character of the temperer's work; of course a temperer never overheats steel, but then the fracture cannot lie.

If a piece of a bar of steel one and a half inches square be heated to circles *b*, and one end be quenched and the other end cooled slowly, we have a different state of affairs. The quenched end will look like this:



The big crystals around the edge from ⅛ to ¼-inch deep will be nearly as big as peas, some of them, some smaller, and the inner part will be what I suppose Mr. Brinnell calls "flaky crystallines," a term that expresses it well.

Some people might call it fine grain, but it isn't.

The big crystals may be picked off with the finger-nail almost, and their corners will scratch glass.

They are represented by circles *b*, and they are the crystals due to that temperature, unaffected by the rate of cooling; it is obvious that the flaky interior is an effect due to the binding of the outside rim, and not to the sudden cooling. The outside edge represents the sudden cooling.

In the ½-inch bar mentioned, it is all outside edge crystals, because the sudden cooling strikes through. The slowly-cooled end of the

1½-inch bar will have what is called a "coarse amorphous" grain; it will be coarser than the center of the quenched end, but not near as coarse as the outside coat of crystals, which alone are due to sudden cooling.

Professor Langley's specific gravity experiments have let in a flood of light on the physics of steel, and if they have not told us why steel hardens, and what carbon has to do with it, and how many forms carbon is capable of assuming; they have told us why steel cracks, and why high steel is brittle; and they have eliminated the words "mystery" and "treacherous" from our steel vocabulary.

Steel is not treacherous. "True as steel" is as sound a metaphor today as it ever was. My friend Mr. Gottlieb need not be afraid of steel if he will get the right kind of steel and then use it right.

Perhaps I ought to say something about the chemistry of steel in answer to some remarks in the discussion, but it would be useless. I do not know the proper relation between the chemistry and physics of steel, and I know of but one person who does know it, and that great "standard" bearer is so entirely alone in his glory that I would not dare to approach him.

Let those who are interested study Mr. A. M. Howe's valuable and interesting work that is being published now in the *Engineering and Mining Journal*. Mr. Howe discusses some two thousand tests by the light of their chemistry, and he finds the fog so thick that he lays down his pen in weariness. If so bright an investigator as Mr. Howe cannot elucidate the subject, it were useless for me to try.